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# ESSAYS

# SCIENTIFIC, POLITICAL, AND SPECULATIVE

BY
HERBERT SPENCER

LIBRARY EDITION
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VOL. I

NEW YORK
D. APPLETON AND COMPANY
1891

*Authorized Edition.*

# PREFACE.

EXCEPTING those which have appeared as articles in periodicals during the last eight years, the essays here gathered together were originally re-published in separate volumes at long intervals. The first volume appeared in December 1857; the second in November 1863; and the third in February 1874. By the time the original editions of the first two had been sold, American reprints, differently entitled and having the essays differently arranged, had been produced; and, for economy's sake, I have since contented myself with importing successive supplies printed from the American stereotype plates. Of the third volume, however, supplies have, as they were required, been printed over here, from plates partly American and partly English. The completion of this final edition of course puts an end to this make-shift arrangement.

The essays above referred to as having been written since 1882, are now incorporated with those previously re-published. There are seven of them; namely—"Morals and Moral Sentiments," "The Factors of Organic Evolution," "Professor Green's Explanations," "The Ethics of Kant," "Absolute Political Ethics," "From Freedom to Bondage," and "The Americans." As well as these large additions there are small additions, in the shape of postscripts to various essays—one to "The Constitution of the Sun," one to "The Philosophy of Style," one to "Railway Morals," one to "Prison Ethics," and one to "The Origin and Function of Music:" which last is about equal in length to the original essay. Changes have been made in many of the essays: in some cases by omitting passages and in other cases by including new ones. Especially the essay on "The Nebular Hypothesis" may be named as one which, though unchanged in essentials, has been much altered by additions and subtractions, and by bringing its statements up to date; so that it has been in large measure re-cast. Beyond these respects in which this final edition differs from preceding editions, it differs in having undergone a verification of its references and quotations, as well as a second verbal revision.

Naturally the fusion of three separate series of essays into one series, has made needful a general re-arrangement. Whether to follow the order of time or the order of subjects was a question which presented itself; and, as neither alternative promised satisfactory results, I eventually decided to compromise—to follow partly the one order and partly the other. The first volume is made up of essays in which the idea of evolution, general or special, is dominant. In the second volume essays dealing with philosophical questions, with abstract and concrete science, and with æsthetics, are brought together; but though all of them are tacitly evolutionary, their evolutionism is an incidental rather than a necessary trait. The ethical, political, and social essays composing the third volume, though mostly written from the evolution point of view, have for their more immediate purposes the enunciation of doctrines which are directly practical in their bearings. Meanwhile, within each volume the essays are arranged in order of time: not indeed strictly, but so far as consists with the requirements of sub-classing.

Beyond the essays included in these three volumes, there remain several which I have not thought it well to include —in some cases because of their personal character, in other cases because of their relative unimportance, and in yet other cases because they would scarcely be understood in the absence of the arguments to which they are replies. But for the convenience of any who may wish to find them, I append their titles and places of publication. These are as follows:—"Retrogressive Religion," in *The Nineteenth Century* for July 1884; "Last Words about Agnosticism and the Religion of Humanity," in *The Nineteenth Century* for November 1884; a note to Prof. Cairns' Critique on the *Study of Sociology*, in *The Fortnightly Review*, for February 1875; "A Short Rejoinder" [to Mr. J. F. McLennan], *Fortnightly Review*, June 1877; "Prof. Goldwin Smith as a Critic," *Contemporary Review*, March 1882; "A Rejoinder to M. de Laveleye," *Contemporary Review*, April 1885.

LONDON, *December*, 1890.

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# THE DEVELOPMENT HYPOTHESIS.

[*Originally published in* The Leader, *for March* 20, 1852. *Brief though it is, I place this essay before the rest, partly because with the exception of a similarly-brief essay on "Use and Beauty", it came first in order of time, but chiefly because it came first in order of thought, and struck the keynote of all that was to follow.*]

In a debate upon the development hypothesis, lately narrated to me by a friend, one of the disputants was described as arguing that as, in all our experience, we know no such phenomenon as transmutation of species, it is unphilosophical to assume that transmutation of species ever takes place. Had I been present I think that, passing over his assertion, which is open to criticism, I should have replied that, as in all our experience we have never known a species *created*, it was, by his own showing, unphilosophical to assume that any species ever had been created.

Those who cavalierly reject the Theory of Evolution as not being adequately supported by facts, seem to forget that their own theory is supported by no facts at all. Like the majority of men who are born to a given belief, they demand the most rigorous proof of any adverse belief, but assume that their own needs none. Here we find, scattered over the globe, vegetable and animal organisms numbering, of the one kind (according to Humboldt), some 320,000 species, and of the other, some 2,000,000 species (see Carpenter); and if to these we add the numbers of animal and vegetable species which have become extinct, we may safely estimate the number of species that have existed, and are existing, on the Earth, at not less than *ten millions*. Well, which is the most rational theory about these ten millions of species? Is it most likely that there have been ten millions of special creations? or is it most likely that, by continual modifications due to change of circumstances, ten millions of varieties have been produced, as varieties are being produced still?

Doubtless many will reply that they can more easily conceive ten millions of special creations to have taken place, than they can conceive that ten millions of varieties have arisen by successive modifications. All such, however, will find, on inquiry, that they are under an illusion. This is one of the many cases in which men do not really believe, but rather *believe they believe.* It is not that they can truly conceive ten millions of special creations to have taken place, but that they *think they can do so.* Careful introspection will show them that they have never yet realized to themselves the creation of even *one* species. If they have formed a definite conception of the process, let them tell us how a new species is constructed, and how it makes its appearance. Is it thrown down from the clouds? or must we hold to the notion that it struggles up out of the ground? Do its limbs and viscera rush together from all the points of the compass? or must we receive the old Hebrew idea, that God takes clay and moulds a new creature? If they say that a new creature is produced in none of these modes, which are too absurd to be believed, then they are required to describe the mode in which a new creature *may* be produced—a mode which does *not* seem absurd; and such a mode they will find that they neither have conceived nor can conceive.

Should the believers in special creations consider it unfair thus to call upon them to describe how special creations take place, I reply that this is far less than they demand from the supporters of the Development Hypothesis. They are merely asked to point out a *conceivable* mode. On the other hand, they ask, not simply for a *conceivable* mode, but for the *actual* mode. They do not say—Show us how this *may* take place; but they say—Show us how this *does* take place. So far from its being unreasonable to put the above question, it would be reasonable to ask not only for a *possible* mode of special creation, but for an *ascertained* mode; seeing that this is no greater a demand than they make upon their opponents.

And here we may perceive how much more defensible the new doctrine is than the old one. Even could the supporters of the Development Hypothesis merely show that the origination of species by the process of modification is conceivable, they would be in a better position than their opponents. But they can do much more than this. They can show that the process of modification has effected, and is effecting, decided changes in all organisms subject to modifying influences. Though, from the impossibility of getting at a sufficiency of facts, they are unable to trace the many phases through which any existing species has passed in arriving at its present form, or to identify the influences which caused the successive modifications; yet, they can show that any existing species—animal or vegetable—when placed under conditions different from its previous ones, *immediately begins to undergo certain changes fitting it for the new conditions.* They can show that in successive generations these changes continue; until, ultimately, the new conditions become the natural ones. They can show that in cultivated plants, in domesticated animals, and in the several races of men, such alterations have taken place. They can show that the degrees of difference so produced are often, as in dogs, greater than those on which distinctions of species are in other cases founded. They can show that it is a matter of dispute whether some of these modified forms are varieties or separate species. They can show, too, that the changes daily taking place in ourselves—the facility that attends long practice, and the loss of aptitude that begins when practice ceases—the strengthening of passions habitually gratified, and the weakening of those habitually curbed—the development of every faculty, bodily, moral, or intellectual, according to the use made of it—are all explicable on this same principle. And thus they can show that throughout all organic nature there *is* at work a modifying influence of the kind they assign as the cause of these specific differences: an influence which, though slow in its action, does, in time, if the circumstances demand it, produce marked changes—an influence which, to all appearance, would produce in the millions of years, and under the great varieties of condition which geological records imply, any amount of change.

Which, then, is the most rational hypothesis?—that of special creations which has neither a fact to support it nor is even definitely conceivable; or that of modification, which is not only definitely conceivable, but is countenanced by the habitudes of every existing organism?

That by any series of changes a protozoon should ever become a mammal, seems to those who are not familiar with zoology, and who have not seen how clear becomes the relationship between the simplest and the most complex forms when intermediate forms are examined, a very grotesque notion. Habitually looking at things rather in their statical aspect than in their dynamical aspect, they never realize the fact that, by small increments of modification, any amount of modification may in time be generated. That surprise which they feel on finding one whom they last saw as a boy, grown into a man, becomes incredulity when the degree of change is greater. Nevertheless, abundant instances are at hand of the mode in which we may pass to the most diverse forms by insensible gradations. Arguing the matter some time since with a learned professor, I illustrated my position thus:—You admit that there is no apparent relationship between a circle and an hyperbola. The one is a finite curve; the other is an infinite one. All parts of the one are alike; of the other no parts are alike [save parts on its opposite sides]. The one incloses a space; the other will not inclose a space though produced for ever. Yet opposite as are these curves in all their properties, they may be connected together by a series of intermediate curves, no one of which differs from the adjacent ones in any appreciable degree. Thus, if a cone be cut by a plane at right angles to its axis we get a circle. If, instead of being perfectly at right angles, the plane subtends with the axis an angle of 89° 59′, we have an ellipse which no human eye, even when aided by an accurate pair of compasses, can distinguish from a circle. Decreasing the angle minute by minute, the ellipse becomes first perceptibly eccentric, then manifestly so, and by and by acquires so immensely elongated a form, as to bear no recognizable resemblance to a circle. By continuing this process, the ellipse passes insensibly into a parabola; and, ultimately, by still further diminishing the angle, into an hyperbola. Now here we have four different species of curve—circle, ellipse, parabola, and hyperbola—each having its peculiar properties and its separate equation, and the first and last of which are quite opposite in nature, connected together as members of one series, all producible by a single process of insensible modification.

But the blindness of those who think it absurd to suppose that complex organic forms may have arisen by successive modifications out of simple ones, becomes astonishing when we remember that complex organic forms are daily being thus produced. A tree differs from a seed immeasurably in every respect—in bulk, in structure, in colour, in form, in chemical composition: differs so greatly that no visible resemblance of any kind can be pointed out between them. Yet is the one changed in the course of a few years into the other: changed so gradually, that at no moment can it be said—Now the seed ceases to be, and the tree exists. What can be more widely contrasted than a newly-born child and the small, semi-transparent spherule constituting the human ovum? The infant is so complex in structure that a cyclopædia is needed to describe its constituent parts. The germinal vesicle is so simple that it may be defined in a line. Nevertheless a few months suffice to develop the one out of the other; and that, too, by a series of modifications so small, that were the embryo examined at successive minutes, even a microscope would with difficulty disclose any sensible changes. That the uneducated and the ill-educated should think the hypothesis that all races of beings, man inclusive, may in process of time have been evolved from the simplest monad, a ludicrous one, is not to be wondered at. But for the physiologist, who knows that every individual being *is* so evolved—who knows, further, that in their earliest condition the germs of all plants and animals whatever are so similar, "that there is no appreciable distinction amongst them, which would enable it to be determined whether a particular molecule is the germ of a Conferva or of an Oak, of a Zoophyte or of a Man;" *—for him to make a difficulty of the matter is inexcusable. Surely if a single cell may, when subjected to certain influences, become a man in the space of twenty years; there is nothing absurd in the hypothesis that under certain other influences, a cell may, in the course of millions of years, give origin to the human race.

We have, indeed, in the part taken by many scientific men in this controversy of "Law *versus* Miracle," a good illustration of the tenacious vitality of superstitions. Ask one of our leading geologists or physiologists whether he believes in the Mosaic account of the creation, and he will take the question as next to an insult. Either he rejects the narrative entirely, or understands it in some vague nonnatural sense. Yet one part of it he unconsciously adopts; and that, too, literally. For whence has he got this notion of "special creations," which he thinks so reasonable, and fights for so vigorously? Evidently he can trace it back to no other source than this myth which he repudiates. He has not a single fact in nature to cite in proof of it; nor is he prepared with any chain of reasoning by which it may be established. Catechize him, and he will be forced to confess that the notion was put into his mind in childhood as part of a story which he now thinks absurd. And why, after rejecting all the rest of the story, he should strenuously defend this last remnant of it, as though he had received it on valid authority, he would be puzzled to say.

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* Carpenter, *Principles of Comparative Physiology*, p. 474.

# PROGRESS: ITS LAW AND CAUSE.

[*First published in* The Westminster Review *for April,* 1857. *Though the ideas and illustrations contained in this essay were eventually incorporated in* First Principles, *yet I think it well here to reproduce it as exhibiting the form under which the General Doctrine of Evolution made its first appearance.*]

THE current conception of progress is shifting and indefinite. Sometimes it comprehends little more than simple growth—as of a nation in the number of its members and the extent of territory over which it spreads. Sometimes it has reference to quantity of material products—as when the advance of agriculture and manufactures is the topic. Sometimes the superior quality of these products is contemplated; and sometimes the new or improved appliances by which they are produced. When, again, we speak of moral or intellectual progress, we refer to states of the individual or people exhibiting it; while, when the progress of Science, or Art, is commented upon, we have in view certain abstract results of human thought and action. Not only, however, is the current conception of progress more or less vague, but it is in great measure erroneous. It takes in not so much the reality of progress as its accompaniments—not so much the substance as the shadow. That progress in intelligence seen during the growth of the child into the man, or the savage into the philosopher, is commonly regarded as consisting in the greater number of facts known and laws understood; whereas the actual progress consists in those internal modifications of which this larger knowledge is the expression. Social progress is supposed to consist in the making of a greater quantity and variety of the articles required for satisfying men's wants; in the increasing security of person and property; in widening freedom of action; whereas, rightly understood, social progress consists in those changes of structure in the social organism which have entailed these consequences. The current conception is a teleological one. The phenomena are contemplated solely as bearing on human happiness. Only those changes are held to constitute progress which directly or indirectly tend to heighten human happiness; and they are thought to constitute progress simply *because* they tend to heighten human happiness. But rightly to understand progress, we must learn the nature of these changes, considered apart from our interests. Ceasing, for example, to regard the successive geological modifications that have taken place in the Earth, as modifications that have gradually fitted it for the habitation of Man, and as *therefore* constituting geological progress, we must ascertain the character common to these modifications—the law to which they all conform. And similarly in every other case. Leaving out of sight concomitants and beneficial consequences, let us ask what progress is in itself.

In respect to that progress which individual organisms display in the course of their evolution, this question has been answered by the Germans. The investigations of Wolff, Goethe, and von Baer, have established the truth that the series of changes gone through during the development of a seed into a tree, or an ovum into an animal, constitute an advance from homogeneity of structure to heterogeneity of structure. In its primary stage, every germ consists of a substance that is uniform throughout, both in texture and chemical composition. The first step is the appearance of a difference between two parts of this substance; or, as the phenomenon is called in physiological language, a differentiation. Each of these differentiated divisions presently begins itself to exhibit some contrast of parts: and by and by these secondary differentiations become as definite as the original one. This process is continuously repeated—is simultaneously going on in all parts of the growing embryo; and by endless such differentiations there is finally produced that complex combination of tissues and organs constituting the adult animal or plant. This is the history of all organisms whatever. It is settled beyond dispute that organic progress consists in a change from the homogeneous to the heterogeneous.

Now, we propose in the first place to show, that this law of organic progress is the law of all progress. Whether it be in the development of the Earth, in the development of Life upon its surface, in the development of Society, of Government, of Manufactures, of Commerce, of Language, Literature, Science, Art, this same evolution of the simple into the complex, through successive differentiations, holds throughout. From the earliest traceable cosmical changes down to the latest results of civilization, we shall find that the transformation of the homogeneous into the heterogeneous, is that in which progress essentially consists.

With the view of showing that *if* the Nebular Hypothesis be true, the genesis of the solar system supplies one illustration of this law, let us assume that the matter of which the sun and planets consist was once in a diffused form; and that from the gravitation of its atoms there resulted a gradual concentration. By the hypothesis, the solar system in its nascent state existed as an indefinitely extended and nearly homogeneous medium—a medium almost homogeneous in density, in temperature, and in other physical attributes. The first change in the direction of increased aggregation, brought a contrast in density and a contrast in temperature, between the interior and the exterior of this mass. Simultaneously the drawing in of outer parts caused motions ending in rotation round a centre with various angular velocities. These differentiations increased in number and degree until there was evolved the organized group of sun, planets, and satellites, which we now know—a group which presents numerous contrasts of structure and action among its members. There are the immense contrasts between the sun and the planets, in bulk and in weight; as well as the subordinate contrasts between one planet and another, and between the planets and their satellites. There is the similarly-marked contrast between the sun as almost stationary (relatively to the other members of the Solar System), and the planets as moving round him with great velocity: while there are the secondary contrasts between the velocities and periods of the several planets, and between their simple revolutions and the double ones of their satellites, which have to move round their primaries while moving round the sun. There is the yet further strong contrast between the sun and the planets in respect of temperature; and there is good reason to suppose that the planets and satellites differ from each other in their proper heats, as well as in the amounts of heat they receive from the sun. When we bear in mind that, in addition to these various contrasts, the planets and satellites also differ in respect to their distances from each other and their primary; in respect to the inclinations of their orbits, the inclinations of their axes, their times of rotation on their axes, their specific gravities, and their physical constitutions; we see what a high degree of heterogeneity the solar system exhibits, when compared with the almost complete homogeneity of the nebulous mass out of which it is supposed to have originated.

Passing from this hypothetical illustration, which must be taken for what it is worth, without prejudice to the general argument, let us descend to a more certain order of evidence. It is now generally agreed among geologists and physicists that the Earth was at one time a mass of molten matter. If so, it was at that time relatively homogeneous in consistence, and, in virtue of the circulation which takes place in heated fluids, must have been comparatively homogeneous in temperature; and it must have been surrounded by an atmosphere consisting partly of the elements of air and water, and partly of those various other elements which are among the more ready to assume gaseous forms at high temperatures. That slow cooling by radiation which is still going on at an inappreciable rate, and which, though originally far more rapid than now, necessarily required an immense time to produce any decided change, must ultimately have resulted in the solidification of the portion most able to part with its heat—namely, the surface. In the thin crust thus formed we have the first marked differentiation. A still further cooling, a consequent thickening of this crust, and an accompanying deposition of all solidifiable elements contained in the atmosphere, must finally have been followed by the condensation of the water previously existing as vapour. A second marked differentiation must thus have arisen; and as the condensation must have taken place on the coolest parts of the surface—namely, about the poles—there must thus have resulted the first geographical distinction of parts. To these illustrations of growing heterogeneity, which, though deduced from known physical laws, may be regarded as more or less hypothetical, Geology adds an extensive series that have been inductively established. Investigations show that the Earth has been continually becoming more heterogeneous in virtue of the multiplication of sedimentary strata which form its crust; also, that it has been becoming more heterogeneous in respect of the composition of these strata, the later of which, being made from the detritus of the earlier, are many of them rendered highly complex by the mixture of materials they contain; and further, that this heterogeneity has been vastly increased by the actions of the Earth's still molten nucleus upon its envelope, whence have resulted not only many kinds of igneous rocks, but the tilting up of sedimentary strata at all angles, the formation of faults and metallic veins, the production of endless dislocations and irregularities. Yet again, geologists teach us that the Earth's surface has been growing more varied in elevation —that the most ancient mountain systems are the smallest, and the Andes and Himalayas the most modern; while in all probability there have been corresponding changes in the bed of the ocean. As a consequence of these ceaseless differentiations, we now find that no considerable portion of the Earth's exposed surface is like any other portion, either in contour, in geologic structure, or in chemical composition; and that in most parts it changes from mile to mile in all these characters. Moreover, there has been simultaneously going on a differentiation of climates. As fast as the Earth cooled and its crust solidified, there arose appreciable differences in temperature between those parts of its surface more exposed to the sun and those less exposed. As the cooling progressed, these differences became more pronounced; until there finally resulted those marked contrasts between regions of perpetual ice and snow, regions where winter and summer alternately reign for periods varying according to the latitude, and regions where summer follows summer with scarcely an appreciable variation. At the same time the many and varied elevations and subsidences of portions of the Earth's crust, bringing about the present irregular distribution of land and sea, have entailed modifications of climate beyond those dependent on latitude; while a yet further series of such modifications have been produced by increasing differences of elevation in the land, which have in sundry places brought arctic, temperate, and tropical climates to within a few miles of one another. And the general outcome of these changes is, that not only has every extensive region its own meteorologic conditions, but that every locality in each region differs more or less from others in those conditions; as in its structure, its contour, its soil. Thus, between our existing Earth, the phenomena of whose crust neither geographers, geologists, mineralogists, nor meteorologists have yet enumerated, and the molten globe out of which it was evolved, the contrast in heterogeneity is extreme.

When from the Earth itself we turn to the plants and animals which have lived, or still live, upon its surface, we find ourselves in some difficulty from lack of facts. That every existing organism has been developed out of the simple into the complex, is indeed the first established truth of all; and that every organism which existed in past times was similarly developed, is an inference no physiologist will hesitate to draw. But when we pass from individual forms of life to Life in general, and inquire whether the same law is seen in the *ensemble* of its manifestations,—whether modern plants and animals are of more heterogeneous structure than ancient ones, and whether the Earth's present Flora and Fauna are more heterogeneous than the Flora and Fauna of the past,—we find the evidence so fragmentary, that every conclusion is open to dispute. Three-fifths of the Earth's surface being covered by water; a great part of the exposed land being inaccessible to, or untravelled by, the geologist; the greater part of the remainder having been scarcely more than glanced at; and even the most familiar portions, as England, having been so imperfectly explored that a new series of strata has been added within these four years,—it is impossible for us to say with certainty what creatures have, and what have not, existed at any particular period. Considering the perishable nature of many of the lower organic forms, the metamorphosis of numerous sedimentary strata, and the great gaps occurring among the rest, we shall see further reason for distrusting our deductions. On the one hand, the repeated discovery of vertebrate remains in strata previously supposed to contain none,—of reptiles where only fish were thought to exist,—of mammals where it was believed there were no creatures higher than reptiles,—renders it daily more manifest how small is the value of negative evidence. On the other hand, the worthlessness of the assumption that we have discovered the earliest, or anything like the earliest, organic remains, is becoming equally clear. That the oldest known sedimentary rocks have been greatly changed by igneous action, and that still older ones have been totally transformed by it, is becoming undeniable. And the fact that sedimentary strata earlier than any we know, have been melted up, being admitted, it must also be admitted that we cannot say how far back in time this destruction of sedimentary strata has been going on. Thus the title *Palæozoic*, as applied to the earliest known fossiliferous strata, involves a *petitio principii;* and, for aught we know to the contrary, only the last few chapters of the Earth's biological history may have come down to us. On neither side, therefore, is the evidence conclusive. Nevertheless we cannot but think that, scanty as they are, the facts, taken altogether, tend to show both that the more heterogeneous organisms have been evolved in the later geologic periods, and that Life in general has been more heterogeneously manifested as time has advanced. Let us cite, in illustration, the one case of the *Vertebrata.* The earliest known vertebrate remains are those of Fishes; and Fishes are the most homogeneous of the vertebrata. Later and more heterogeneous are Reptiles. Later still, and more heterogeneous still, are Birds and Mammals. If it be said that the Palæozoic deposits, not being estuary deposits, are not likely to contain the remains of terrestrial vertebrata, which may nevertheless have existed at that era, we reply that we are merely pointing to the leading facts, *such as they are.* But to avoid any such criticism, let us take the mammalian subdivision only. The earliest known remains of mammals are those of small marsupials, which are the lowest of the mammalian type; while, conversely, the highest of the mammalian type—Man—is the most recent. The evidence that the vertebrate fauna, as a whole, has become more heterogeneous, is considerably stronger. To the argument that the vertebrate fauna of the Palæozoic period, consisting, so far as we know, entirely of Fishes, was less heterogeneous than the modern vertebrate fauna, which includes Reptiles, Birds, and Mammals, of multitudinous genera, it may be replied, as before, that estuary deposits of the Palæozoic period, could we find them, might contain other orders of vertebrata. But no such reply can be made to the argument that whereas the marine vertebrata of the Palæozoic period consisted entirely of cartilaginous fishes, the marine vertebrata of later periods include numerous genera of osseous fishes; and that, therefore, the later marine vertebrate faunas are more heterogeneous than the oldest known one. Nor, again, can any such reply be made to the fact that there are far more numerous orders and genera of mammalian remains in the tertiary formations than in the secondary formations. Did we wish merely to make out the best case, we might dwell upon the opinion of Dr. Carpenter, who says that "the general facts of Palæontology appear to sanction the belief, that *the same plan* may be traced out in what may be called *the general life of the globe*, as in *the individual life* of every one of the forms of organized being which now people it." Or we might quote, as decisive, the judgment of Professor Owen, who holds that the earlier examples of each group of creatures severally departed less widely from archetypal generality than the later examples—were severally less unlike the fundamental form common to the group as a whole; and thus constituted a less heterogeneous group of creatures. But in deference to an authority for whom we have the highest respect, who considers that the evidence at present obtained does not justify a verdict either way, we are content to leave the question open.*

Whether an advance from the homogeneous to the heterogeneous is or is not displayed in the biological history of the globe, it is clearly enough displayed in the progress of the latest and most heterogeneous creature—Man. It is true alike that, during the period in which the Earth has been peopled, the human organism has grown more heterogeneous among the civilized divisions of the species; and that the species, as a whole, has been growing more heterogeneous in virtue of the multiplication of races and the differentiation of these races from each other. In proof of the first of these positions, we may cite the fact that, in the relative development of the limbs, the civilized man departs more widely from the general type of the placental mammalia than do the lower human races. While often possessing well-developed body and arms, the Australian has very small legs: thus reminding us of the chimpanzee and the gorilla, which present no great contrasts in size between the hind and fore limbs. But in the European, the greater length and massiveness of the legs have become marked—the fore and hind limbs are more heterogeneous. Again, the greater ratio which the cranial bones bear to the facial bones illustrates the same truth. Among the vertebrata in general, progress is marked by an increasing heterogeneity in the vertebral column, and more especially in the segments constituting the skull: the higher forms being distinguished by the relatively larger size of the bones which cover the brain, and the relatively smaller size of those which form the jaws, &c. Now this characteristic, which is stronger in Man than in any other creature, is stronger in the European than in the savage. Moreover, judging from the greater extent and variety of faculty he exhibits, we may infer that the civilized man has also a more complex or heterogeneous nervous system than the uncivilized man: and, indeed, the fact is in part visible in the increased ratio which his cerebrum bears to the subjacent ganglia, as well as in the wider departure from symmetry in its convolutions. If further elucidation be needed, we may find it in every nursery. The infant European has sundry marked points of resemblance to the lower human races; as in the flatness of the alæ of the nose, the depression of its bridge, the divergence and forward opening of the nostrils, the form of the lips, the absence of a frontal sinus, the width between the eyes, the smallness of the legs. Now, as the developmental process by which these traits are turned into those of the adult European, is a continuation of that change from the homogeneous to the heterogeneous displayed during the previous evolution of the embryo, which every anatomist will admit; it follows that the parallel developmental process by which the like traits of the barbarous races have been turned into those of the civilized races, has also been a continuation of the change from the homogeneous to the heterogeneous. The truth of the second position—that Mankind, as a whole, have become more heterogeneous—is so obvious as scarcely to need illustration. Every work on Ethnology, by its divisions and subdivisions of races, bears testimony to it. Even were we to admit the hypothesis that Mankind originated from several separate stocks, it would still remain true, that as, from each of these stocks, there have sprung many now widely-different tribes, which are proved by philological evidence to have had a common origin, the race as a whole is far less homogeneous than it once was. Add to which, that we have, in the Anglo-

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* Since this was written (in 1857) the advance of paleontological discovery, especially in America, has shown conclusively, in respect of certain groups of vertebrates, that higher types have arisen by modifications of lower; so that, in common with others, Prof. Huxley, to whom the above allusion is made, now admits, or rather asserts, biological progression, and, by implication, that there have arisen more heterogeneous organic forms and a more heterogeneous assemblage of organic forms.

Americans, an example of a new variety arising within these few generations; and that, if we may trust to the descriptions of observers, we are likely soon to have another such example in Australia.

On passing from Humanity under its individual form, to Humanity as socially embodied, we find the general law still more variously exemplified. The change from the homogeneous to the heterogeneous is displayed in the progress of civilization as a whole, as well as in the progress of every nation; and is still going on with increasing rapidity. As we see in existing barbarous tribes, society in its first and lowest form is a homogeneous aggregation of individuals having like powers and like functions: the only marked difference of function being that which accompanies difference of sex. Every man is warrior, hunter, fisherman, tool-maker, builder; every woman performs the same drudgeries. Very early, however, in the course of social evolution, there arises an incipient differentiation between the governing and the governed. Some kind of chieftainship seems coeval with the first advance from the state of separate wandering families to that of a nomadic tribe. The authority of the strongest or the most cunning makes itself felt among a body of savages as in a herd of animals, or a posse of schoolboys. At first, however, it is indefinite, uncertain; is shared by others of scarcely inferior power; and is unaccompanied by any difference in occupation or style of living: the first ruler kills his own game, makes his own weapons, builds his own hut, and, economically considered, does not differ from others of his tribe. Gradually, as the tribe progresses, the contrast between the governing and the governed grows more decided. Supreme power becomes hereditary in one family; the head of that family, ceasing to provide for his own wants, is served by others; and he begins to assume the sole office of ruling. At the same time there has been arising a co-ordinate species of government—that of Religion. As all ancient records and traditions prove, the earliest rulers are regarded as divine personages. The maxims and commands they uttered during their lives are held sacred after their deaths, and are enforced by their divinely-descended successors; who in their turns are promoted to the pantheon of the race, here to be worshipped and propitiated along with their predecessors: the most ancient of whom is the supreme god, and the rest subordinate gods. For a long time these connate forms of government—civil and religious—remain closely associated. For many generations the king continues to be the chief priest, and the priesthood to be members of the royal race. For many ages religious law continues to include more or less of civil regulation, and civil law to possess more or less of religious sanction; and even among the most advanced nations these two controlling agencies are by no means completely separated from each other. Having a common root with these, and gradually diverging from them, we find yet another controlling agency—that of Ceremonial usages. All titles of honour are originally the names of the god-king; afterwards of the god and the king; still later of persons of high rank; and finally come, some of them, to be used between man and man. All forms of complimentary address were at first the expressions of submission from prisoners to their conqueror, or from subjects to their ruler, either human or divine—expressions which were afterwards used to propitiate subordinate authorities, and slowly descended into ordinary intercourse. All modes of salutation were once obeisances made before the monarch and used in worship of him after his death. Presently others of the god-descended race were similarly saluted; and by degrees some of the salutations have become the due of all.* Thus, no sooner does the originally-homogeneous social mass differentiate into the governed and the governing parts, than this last exhibits an incipient differentiation into religious and secular—Church and State; while at the same time there begins to be differentiated from both, that less definite species of government which rules our daily intercourse—a species of government which, as we may see in heralds' colleges, in books of the peerage, in masters of ceremonies, is not without a certain embodiment of its own. Each of these is itself subject to successive differentiations. In the course of ages, there arises, as among ourselves, a highly complex political organization of monarch, ministers, lords and commons, with their subordinate administrative departments, courts of justice, revenue offices, &c., supplemented in the provinces by municipal governments, county governments, parish or union governments—all of them more or less elaborated. By its side there grows up a highly complex religious organization, with its various grades of officials, from archbishops down to sextons, its colleges, convocations, ecclesiastical courts, &c.; to all which must be added the ever-multiplying independent sects, each with its general and local authorities. And at the same time there is developed a highly complex aggregation of customs, manners, and temporary fashions, enforced by society at large, and serving to control those minor transactions between man and man which are not regulated by civil and religious law. Moreover, it is to be observed that this increasing heterogeenity in the governmental appliances of each nation, has been accompanied by an increasing heterogeneity in the assemblage of governmental appliances of different nations: all nations being more or less unlike in their political systems and legislation, in their creeds and religious institutions, in their customs and ceremonial usages.

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* For detailed proof of these assertions see essay on "Manners and Fashion."

Simultaneously there has been going on a second differentiation of a more familiar kind; that, namely, by which the mass of the community has been segregated into distinct classes and orders of workers. While the governing part has undergone the complex development above detailed, the governed part has undergone an equally complex development, which has resulted in that minute division of labour characterizing advanced nations. It is needless to trace out this progress from its first stages, up through the caste-divisions of the East and the incorporated guilds of Europe, to the elaborate producing and distributing organization existing among ourselves. It has been an evolution which, beginning with a tribe whose members severally perform the same actions each for himself, ends with a civilized community whose members severally perform different actions for each other; and an evolution which has transformed the solitary producer of any one commodity into a combination of producers who, united under a master, take separate parts in the manufacture of such commodity. But there are yet other and higher phases of this advance from the homogeneous to the heterogeneous in the industrial organization of society. Long after considerable progress has been made in the division of labour among different classes of workers, there is still little or no division of labour among the widely separated parts of the community: the nation continues comparatively homogeneous in the respect that in each district the same occupations are pursued. But when roads and other means of transit become numerous and good, the different districts begin to assume different functions, and to become mutually dependent. The calico manufacture locates itself in this county, the woollen-cloth manufacture in that; silks are produced here, lace there; stockings in one place, shoes in another; pottery, hardware, cutlery, come to have their special towns; and ultimately every locality becomes more or less distinguished from the rest by the leading occupation carried on in it. This subdivision of functions shows itself not only among the different parts of the same nation, but among different nations. That exchange of commodities which free-trade is increasing so largely, will ultimately have the effect of specializing, in a greater or less degree, the industry of each people. So that, beginning with a barbarous tribe, almost if not quite homogeneous in the functions of its members, the progress has been, and still is, towards an economic aggregation of the whole human race; growing ever more heterogeneous in respect of the separate functions assumed by separate nations, the separate functions assumed by the local sections of each nation, the separate functions assumed by the many kinds of makers and traders in each town, and the separate functions assumed by the workers united in producing each commodity.

The law thus clearly exemplified in the evolution of the social organism, is exemplified with equal clearness in the evolution of all products of human thought and action; whether concrete or abstract, real or ideal. Let us take Language as our first illustration.

The lowest form of language is the exclamation, by which an entire idea is vaguely conveyed through a single sound, as among the lower animals. That human language ever consisted solely of exclamations, and so was strictly homogeneous in respect of its parts of speech, we have no evidence. But that language can be traced down to a form in which nouns and verbs are its only elements, is an established fact. In the gradual multiplication of parts of speech out of these primary ones—in the differentiation of verbs into active and passive, of nouns into abstract and concrete—in the rise of distinctions of mood, tense, person, of number and case—in the formation of auxiliary verbs, of adjectives, adverbs, pronouns, prepositions, articles—in the divergence of those orders, genera, species, and varieties of parts of speech by which civilized races express minute modifications of meaning—we see a change from the homogeneous to the heterogeneous. Another aspect under which we may trace the development of language is the divergence of words having common origins. Philology early disclosed the truth that in all languages words may be grouped into families, the members of each of which are allied by their derivation. Names springing from a primitive root, themselves become the parents of other names still further modified. And by the aid of those systematic modes which presently arise, of making derivatives and forming compound terms, there is finally developed a tribe of words so heterogeneous in sound and meaning, that to the uninitiated it seems incredible they should be nearly related. Meanwhile from other roots there are being evolved other such tribes, until there results a language of some sixty thousand or more unlike words, signifying as many unlike objects, qualities, acts. Yet another way in which language in general advances from the homogeneous to the heterogeneous, is in the multiplication of languages. Whether all languages have grown from one stock, or whether, as some philologists think, they have grown from two or more stocks, it is clear that since large groups of languages, as the Indo-European, are of one parentage, they have become distinct through a process of continuous divergence. The same diffusion over the Earth's surface which has led to differentiations of race, has simultaneously led to differentiations of speech: a truth which we see further illustrated in each nation by the distinct dialects found in separate districts. Thus the progress of Language conforms to the general law, alike in the evolution of languages, in the evolution of families of words, and in the evolution of parts of speech.

On passing from spoken to written language, we come upon several classes of facts, having similar implications. Written language is connate with Painting and Sculpture; and at first all three are appendages of Architecture, and have a direct connection with the primary form of all Government—the theocratic. Merely noting by the way the fact that sundry wild races, as for example the Australians and the tribes of South Africa, are given to depicting personages and events upon the walls of caves, which are probably regarded as sacred places, let us pass to the case of the Egyptians. Among them, as also among the Assyrians, we find mural paintings used to decorate the temple of the god and the palace of the king (which were, indeed, originally identical); and as such they were governmental appliances in the same sense as state-pageants and religious feasts were. They were governmental appliances in another way: representing as they did the worship of the god, the triumphs of the god-king, the submission of his subjects, and the punishment of the rebellious. Further, they were governmental, as being the products of an art reverenced by the people as a sacred mystery. From the habitual use of this pictorial representation there grew up the but-slightly-modified practice of picture-writing—a practice which was found still extant among North American peoples at the time they were discovered. By abbreviations analogous to those still going on in our own written language, the most frequently-recurring of these pictured figures were successively simplified; and ultimately there grew up a system of symbols, most of which had but distant resemblances to the things for which they stood. The inference that the hieroglyphics of the Egyptians were thus produced, is confirmed by the fact that the picture-writing of the Mexicans was found to have given birth to a like family of ideographic forms; and among them, as among the Egyptians, these had been partially differentiated into the *kuriological* or imitative, and the *tropical* or symbolic; which were, however, used together in the same record. In Egypt, written language underwent a further differentiation, whence resulted the *hieratic* and the *epistolographic* or *enchorial*; both of which are derived from the original hieroglyphic. At the same time we find that for the expression of proper names, which could not be otherwise conveyed, signs having phonetic values were employed; and though it is alleged that the Egyptians never achieved complete alphabetic writing, yet it can scarcely be doubted that these phonetic symbols, occasionally used in aid of their ideographic ones, were the germs of an alphabetic system. Once having become separate from hieroglyphics, alphabetic writing itself underwent numerous differentiations — multiplied alphabets were produced; between most of which, however, more or less connection can still be traced. And in each civilized nation there has now grown up, for the representation of one set of sounds, several sets of written signs used for distinct purposes. Finally, from writing diverged printing; which, uniform in kind as it was at first, has since become multiform.

While written language was passing through its first stages of development, the mural decoration which contained its root was being differentiated into Painting and Sculpture. The gods, kings, men, and animals represented, were originally marked by indented outlines and coloured. In most cases these outlines were of such depth, and the object they circumscribed so far rounded and marked out in its leading parts, as to form a species of work intermediate between intaglio and bas-relief. In other cases we see an advance upon this: the raised spaces between the figures being chiselled off, and the figures themselves appropriately tinted, a painted bas-relief was produced. The restored Assyrian architecture at Sydenham exhibits this style of art carried to greater perfection—the persons and things represented, though still barbarously coloured, are carved out with more truth and in greater detail: and in the winged lions and bulls used for the angles of gateways, we may see a considerable advance towards a completely sculptured figure; which, nevertheless, is still coloured, and still forms part of the building. But while in Assyria the production of a statue proper seems to have been little, if at all, attempted, we may trace in Egyptian art the gradual separation of the sculptured figure from the wall. A walk through the collection in the British

Museum shows this; while at the same time it affords an opportunity of observing the traces which the independent statues bear of their derivation from bas-relief: seeing that nearly all of them not only display that fusion of the legs with one another and of the arms with the body which is characteristic of bas-relief, but have the back united from head to foot with a block which stands in place of the original wall. Greece repeated the leading stages of this progress. On the friezes of Greek Temples, were coloured bas-reliefs representing sacrifices, battles, processions, games—all in some sort religious. The pediments contained painted sculptures more or less united with the tympanum, and having for subjects the triumphs of gods or heroes. Even statues definitely separated from buildings were coloured; and only in the later periods of Greek civilization does the differentiation of Sculpture from Painting appear to have become complete. In Christian art we may trace a parallel re-genesis. All early works of art throughout Europe were religious in subject—represented Christs, crucifixions, virgins, holy families, apostles, saints. They formed integral parts of church architecture, and were among the means of exciting worship; as in Roman Catholic countries they still are. Moreover, the sculptured figures of Christ on the cross, of virgins, of saints, were coloured; and it needs but to call to mind the painted madonnas still abundant in continental churches and highways, to perceive the significant fact that Painting and Sculpture continue in closest connection with each other where they continue in closest connection with their parent. Even when Christian sculpture became differentiated from painting, it was still religious and governmental in its subjects—was used for tombs in churches and statues of kings; while, at the same time, painting, where not purely ecclesiastical, was applied to the decoration of palaces, and besides representing royal personages, was mostly devoted to sacred legends. Only in recent times have painting and sculpture become quite separate and mainly secular. Only within these few centuries has Painting been divided into historical, landscape, marine, architectural, genre, animal, still-life, &c.; and Sculpture grown heterogeneous in respect of the variety of real and ideal subjects with which it occupies itself.

Strange as it seems then, we find that all forms of written language, of Painting, and of Sculpture, have a common root in the politico-religious decorations of ancient temples and palaces. Little resemblance as they now have, the landscape that hangs against the wall, and the copy of the *Times* lying on the table, are remotely akin. The brazen face of the knocker which the postman has just lifted, is related not only to the woodcuts of the *Illustrated London News* which he is delivering, but to the characters of the *billet-doux* which accompanies it. Between the painted window, the prayer-book on which its light falls, and the adjacent monument, there is consanguinity. The effigies on our coins, the signs over shops, the coat of arms outside the carriage panel, and the placards inside the omnibus, are, in common with dolls and paper-hangings, lineally descended from the rude sculpture-paintings in which ancient peoples represented the triumphs and worship of their god-kings. Perhaps no example can be given which more vividly illustrates the multiplicity and heterogeneity of the products that in course of time may arise by successive differentiations from a common stock.

Before passing to other classes of facts, it should be observed that the evolution of the homogeneous into the heterogeneous is displayed not only in the separation of Painting and Sculpture from Architecture and from each other, and in the greater variety of subjects they embody, but it is further shown in the structure of each work. A modern picture or statue is of far more heterogeneous nature than an ancient one. An Egyptian sculpture-fresco usually represents all its figures as at the same distance from the eye; and so is less heterogeneous than a painting that represents them as at various distances from the eye. It exhibits all objects as exposed to the same degree of light; and so is less heterogeneous than a painting which exhibits its different objects and different parts of each object as in different degrees of light. It uses chiefly the primary colours, and these in their full intensities; and so is less heterogeneous than a painting which, introducing the primary colours but sparingly, employs numerous intermediate tints, each of heterogeneous composition, and differing from the rest not only in quality but in strength. Moreover, we see in these early works great uniformity of conception. The same arrangement of figures is perpetually reproduced—the same actions, attitudes, faces, dresses. In Egypt the modes of representation were so fixed that it was sacrilege to introduce a novelty. The Assyrian bas-reliefs display parallel characters. Deities, kings, attendants, winged-figures and animals, are time after time depicted in like positions, holding like implements, doing like things, and with like expression or non-expression of face. If a palm-grove is introduced, all the trees are of the same height, have the same number of leaves, and are equidistant. When water is imitated, each wave is a counterpart of the rest; and the fish, almost always of one kind, are evenly distributed over the surface. The beards of the kings, the gods, and the winged-figures, are everywhere similar; as are the manes of the lions, and equally so those of the horses. Hair is represented throughout by one form of curl. The king's beard is quite architecturally built up of compound tiers of uniform curls, alternating with twisted tiers placed in a transverse direction, and arranged with perfect regularity; and the terminal tufts of the bulls' tails are represented in exactly the same manner. Without tracing out analogous facts in early Christian art, in which, though less striking, they are still visible, the advance in heterogeneity will be sufficiently manifest on remembering that in the pictures of our own day the composition is endlessly varied; the attitudes, faces, expressions, unlike; the subordinate objects different in sizes, forms, textures; and more or less of contrast even in the smallest details. Or, if we compare an Egyptian statue, seated bolt upright on a block, with hands on knees, fingers parallel, eyes looking straight forward, and the two sides perfectly symmetrical in every particular, with a statue of the advanced Greek school or the modern school, which is asymmetrical in respect of the attitude of the head, the body, the limbs, the arrangement of the hair, dress, appendages, and in its relations to neighbouring objects, we shall see the change from the homogeneous to the heterogeneous clearly manifested.

In the co-ordinate origin and gradual differentiation of Poetry, Music, and Dancing, we have another series of illustrations. Rhythm in words, rhythm in sounds, and rhythm in motions, were in the beginning parts of the same thing, and have only in process of time become separate things. Among existing barbarous tribes we find them still united. The dances of savages are accompanied by some kind of monotonous chant, the clapping of hands, the striking of rude instruments: there are measured movements, measured words, and measured tones. The early records of historic races similarly show these three forms of metrical action united in religious festivals. In the Hebrew writings we read that the triumphal ode composed by Moses on the defeat of the Egyptians, was sung to an accompaniment of dancing and timbrels. The Israelites danced and sung "at the inauguration of the golden calf. And as it is generally agreed that this representation of the Deity was borrowed from the mysteries of Apis, it is probable that the dancing was copied from that of the Egyptians on those occasions." Again, in Greece the like relation is everywhere seen: the original type being there, as probably in other cases, a simultaneous chanting and mimetic representation of the life and adventures of the hero or the god.

The Spartan dances were accompanied by hymns and songs; and in general the Greeks had "no festivals or religious assemblies but what were accompanied with songs and dances"—both of them being forms of worship used before altars. Among the Romans, too, there were sacred dances: the Salian and Lupercalian being named as of that kind. And even in Christian countries, as at Limoges, in comparatively recent times, the people have danced in the choir in honour of a saint. The incipient separation of these once-united arts from each other and from religion, was early visible in Greece. Probably diverging from dances partly religious, partly warlike, as the Corybantian, came the war-dances proper, of which there were various kinds. Meanwhile Music and Poetry, though still united, came to have an existence separate from Dancing. The primitive Greek poems, religious in subject, were not recited but chanted; and though at first the chant of the poet was accompanied by the dance of the chorus, it ultimately grew into independence. Later still, when the poem had been differentiated into epic and lyric—when it became the custom to sing the lyric and recite the epic—poetry proper was born. As during the same period musical instruments were being multiplied, we may presume that music came to have an existence apart from words. And both of them were beginning to assume other forms besides the religious. Facts having like implications might be cited from the histories of later times and peoples; as the practices of our own early minstrels, who sang to the harp heroic narratives versified by themselves to music of their own composition: thus uniting the now separate offices of poet, composer, vocalist, and instrumentalist. But, without further illustration, the common origin and gradual differentiation of Dancing, Poetry, and Music will be sufficiently manifest.

The advance from the homogeneous to the heterogeneous is displayed not only in the separation of these arts from each other and from religion, but also in the multiplied differentiations which each of them afterwards undergoes. Not to dwell upon the numberless kinds of dancing that have, in course of time, come into use: and not to occupy space in detailing the progress of poetry, as seen in the development of the various forms of metre, of rhyme, and of general organization; let us confine our attention to music as a type of the group. As implied by the customs of still extant barbarous races, the first musical instruments were, without doubt, percussive — sticks, calabashes, tom-toms—and were used simply to mark the time of the dance; and in this constant repetition of the same sound, we see music in its most homogeneous form. The Egyptians had a lyre with three strings. The early lyre of the Greeks had four, constituting their tetrachord. In course of some centuries lyres of seven and eight strings were employed; and, by the expiration of a thousand years, they had advanced to their "great system" of the double octave. Through all which changes there of course arose a greater heterogeneity of melody. Simultaneously there came into use the different modes—Dorian, Ionian, Phrygian, Æolian, and Lydian—answering to our keys; and of these there were ultimately fifteen. As yet, however, there was but little heterogeneity in the time of their music. Instrumental music being at first merely the accompaniment of vocal music, and vocal music being subordinated to words,—the singer being also the poet, chanting his own compositions and making the lengths of his notes agree with the feet of his verses,—there resulted a tiresome uniformity of measure, which, as Dr. Burney says, "no resources of melody could disguise." Lacking the complex rhythm obtained by our equal bars and unequal notes, the only rhythm was that produced by the quantity of the syllables, and was of necessity comparatively monotonous. And further, it may be observed that the chant thus resulting, being like recitative, was much less clearly differentiated from ordinary speech than is our modern song. Nevertheless, in virtue of the extended range of notes in use, the variety of modes, the occasional variations of time consequent on changes of metre, and the multiplication of instruments, music had, towards the close of Greek civilization, attained to considerable heterogeneity—not indeed as compared with our music, but as compared with that which preceded it. Still, there existed nothing but melody: harmony was unknown. It was not until Christian church-music had reached some development, that music in parts was evolved; and then it came into existence through a very unobtrusive differentiation. Difficult as it may be to conceive *a priori* how the advance from melody to harmony could take place without a sudden leap, it is none the less true that it did so. The circumstance which prepared the way for it was the employment of two choirs singing alternately the same air. Afterwards it became the practice—very possibly first suggested by a mistake—for the second choir to commence before the first had ceased; thus producing a fugue. With the simple airs then in use, a partially-harmonious fugue might not improbably thus result: and a very partially-harmonious fugue satisfied the ears of that age, as we know from still preserved examples. The idea having once been given, the composing of airs productive of fugal harmony would naturally grow up, as in some way it *did* grow up, out of this alternate choir-singing. And from the fugue to concerted music of two, three, four, and more parts, the transition was easy. Without pointing out in detail the increasing complexity that resulted from introducing notes of various lengths, from the multiplication of keys, from the use of accidentals, from varieties of time, and so forth, it needs but to contrast music as it is, with music as it was, to see how immense is the increase of heterogeneity. We see this if, looking at music in its *ensemble*, we enumerate its many different genera and species—if we consider the divisions into vocal, instrumental, and mixed; and their subdivisions into music for different voices and different instruments—if we observe the many forms of sacred music, from the simple hymn, the chant, the canon, motet, anthem, &c., up to the oratorio; and the still more numerous forms of secular music, from the ballad up to the serenata, from the instrumental solo up to the symphony. Again, the same truth is seen on comparing any one sample of aboriginal music with a sample of modern music—even an ordinary song for the piano; which we find to be relatively very heterogeneous, not only in respect of the variety in the pitches and in the lengths of the notes, the number of different notes sounding at the same instant in company with the voice, and the variations of strength with which they are sounded and sung, but in respect of the changes of key, the changes of time, the changes of *timbre* of the voice, and the many other modifications of expression. While between the old monotonous dance-chant and a grand opera of our own day, with its endless orchestral complexities and vocal combinations, the contrast in heterogeneity is so extreme that it seems scarcely credible that the one should have been the ancestor of the other.

Were they needed, many further illustrations might be cited. Going back to the early time when the deeds of the god-king were recorded in picture-writings on the walls of temples and palaces, and so constituted a rude literature, we might trace the development of Literature through phases in which, as in the Hebrew Scriptures, it presents in one work theology, cosmogony, history, biography, law, ethics, poetry; down to its present heterogeneous development, in which its separated divisions and subdivisions are so numerous and varied as to defy complete classification. Or we might trace out the evolution of Science; beginning with the era in which it was not yet differentiated from Art, and was, in union with Art, the handmaid of Religion; passing through the era in which the sciences were so few and rudimentary, as to be simultaneously cultivated by the same men; and ending with the era in which the genera and species are so numerous that few can enumerate them, and no one can adequately grasp even one genus. Or we might do the like with Architecture, with the Drama, with Dress. But doubtless the reader is already weary of illustrations; and our promise has been amply fulfilled. Abundant proof has been given that the law of organic development formulated by von Baer, is the law of all development. The advance from the simple to the complex, through a process of successive differentiations, is seen alike in the earliest changes of the Universe to which we can reason our way back, and in the earliest changes which we can inductively establish; it is seen in the geologic and climatic evolution of the Earth; it is seen in the unfolding of every single organism on its surface, and in the multiplication of kinds of organisms; it is seen in the evolution of Humanity, whether contemplated in the civilized individual, or in the aggregate of races; it is seen in the evolution of Society in respect alike of its political, its religious, and its economical organization; and it is seen in the evolution of all those endless concrete and abstract products of human activity which constitute the environment of our daily life. From the remotest past which Science can fathom, up to the novelties of yesterday, that in which progress essentially consists, is the transformation of the homogeneous into the heterogeneous.

And now, must not this uniformity of procedure be a consequence of some fundamental necessity? May we not rationally seek for some all-pervading principle which determines this all-pervading process of things? Does not the universality of the *law* imply a universal *cause?*

That we can comprehend such cause, noumenally considered, is not to be supposed. To do this would be to solve that ultimate mystery which must ever transcend human intelligence. But it still may be possible for us to reduce the law of all progress, above set forth, from the condition of an empirical generalization, to the condition of a rational generalization. Just as it was possible to interpret Kepler's laws as necessary consequences of the law of gravitation; so it may be possible to interpret this law of progress, in its multiform manifestations, as the necessary consequence of some similarly universal principle. As gravitation was assignable as the *cause* of each of the groups of phenomena which Kepler generalized; so may some equally simple attribute of things be assignable as the cause of each of the groups of phenomena generalized in the foregoing pages. We may be able to affiliate all these varied evolutions of the homogeneous into the heterogeneous, upon certain facts of immediate experience, which, in virtue of endless repetition, we regard as necessary.

The probability of a common cause, and the possibility of formulating it, being granted, it will be well, first, to ask what must be the general characteristics of such cause, and in what direction we ought to look for it. We can with certainty predict that it has a high degree of abstractness; seeing that it is common to such infinitely-varied phenomena. We need not expect to see in it an obvious solution of this or that form of progress; because it is equally concerned with forms of progress bearing little apparent resemblance to them: its association with multiform orders of facts, involves its dissociation from any particular order of facts. Being that which determines progress of every kind—astronomic, geologic, organic, ethnologic, social, economic, artistic, &c.—it must be involved with some fundamental trait displayed in common by these; and must be expressible in terms of this fundamental trait. The only obvious respect in which all kinds of progress are alike, is, that they are modes of *change;* and hence, in some characteristic of changes in general, the desired solution will probably be found. We may suspect *a priori* that in some universal law of change lies the explanation of this universal transformation of the homogeneous into the heterogeneous.

Thus much premised, we pass at once to the statement of the law, which is this:—*Every active force produces more than one change—every cause produces more than one effect.*

To make this proposition comprehensible, a few examples must be given. When one body strikes another, that which we usually regard as the effect, is a change of position or motion in one or both bodies. But a moment's thought shows us that this is a very incomplete view of the matter. Besides the visible mechanical result, sound is produced; or, to speak accurately, a vibration in one or both bodies, which is communicated to the surrounding air; and under some circumstances we call this the effect. Moreover, the air has not only been made to undulate, but has had currents caused in it by the transit of the bodies. Further, there is a disarrangement of the particles of the two bodies in the neighbourhood of their point of collision; amounting, in some cases, to a visible condensation. Yet more, this condensation is accompanied by the disengagement of heat. In some cases a spark—that is, light—results, from the incandescence of a portion struck off; and sometimes this incandescence is associated with chemical combination. Thus, by the mechanical force expended in the collision, at least five, and often more, different kinds of changes have been produced. Take, again, the lighting of a candle. Primarily this is a chemical change consequent on a rise of temperature. The process of combination having once been started by extraneous heat, there is a continued formation of carbonic acid, water, &c.—in itself a result more complex than the extraneous heat that first caused it. But accompanying this process of combination there is a production of heat; there is a production of light; there is an ascending column of hot gases generated; there are inflowing currents set going in the surrounding air. Moreover, the complicating of effects does not end here: each of the several changes produced becomes the parent of further changes. The carbonic acid given off will by and by combine with some base; or under the influence of sunshine give up its carbon to the leaf of a plant. The water will modify the hygrometric state of the air around; or, if the current of hot gases containing it comes against a cold body, will be condensed: altering the temperature of the surface it covers. The heat given out melts the subjacent tallow, and expands whatever it warms. The light, falling on various substances, calls forth from them reactions by which its composition is modified; and so divers colours are produced. Similarly even with these secondary actions, which may be traced out into ever-multiplying ramifications, until they become too minute to be appreciated. And thus it is with all changes whatever. No case can be named in which an active force does not evolve forces of several kinds, and each of these, other groups of forces. Universally the effect is more complex than the cause.

Doubtless the reader already foresees the course of our argument. This multiplication of effects, which is displayed in every event of to-day, has been going on from the beginning; and is true of the grandest phenomena of the universe as of the most insignificant. From the law that every active force produces more than one change, it is an inevitable corollary that during the past there has been an ever-growing complication of things. Throughout creation there must have gone on, and must still go on, a never-ceasing transformation of the homogeneous into the heterogeneous. Let us trace this truth in detail.

Without committing ourselves to it as more than a speculation, though a highly probable one, let us again commence with the evolution of the Solar System out of a nebulous medium. The hypothesis is that from the mutual attraction of the molecules of a diffused mass whose form is unsymmetrical, there results not only condensation but rotation. While the condensation and the rate of rotation go on increasing, the approach of the molecules is necessarily accompanied by an increasing temperature. As the temperature rises, light begins to be evolved; and ultimately there results a revolving sphere of fluid matter radiating intense heat and light—a sun. There are reasons for believing that, in consequence of the higher tangential velocity originally possessed by the outer parts of the condensing nebulous mass, there will be occasional detachments of rotating rings; and that, from the breaking up of these nebulous rings, there will arise masses which in the course of their condensation repeat the actions of the parent mass, and so produce planets and their satellites—an inference strongly supported by the still extant rings of Saturn. Should it hereafter be satisfactorily shown that planets and satellites were thus generated, a striking illustration will be afforded of the highly heterogeneous effects produced by the primary homogeneous cause; but it will serve our present purpose to point to the fact that from the mutual attraction of the particles of an irregular nebulous mass there result condensation, rotation, heat, and light.

It follows as a corollary from the Nebular Hypothesis, that the Earth must once have been incandescent; and whether the Nebular Hypothesis be true or not, this original incandescence of the Earth is now inductively established—or, if not established, at least rendered so highly probable that it is an accepted geological doctrine. Let us look first at the astronomical attributes of this once molten globe. From its rotation there result the oblateness of its form, the alternations of day and night, and (under the influence of the moon and in a smaller degree the sun) the tides, aqueous and atmospheric. From the inclination of its axis, there result the many differences of the seasons, both simultaneous and successive, that pervade its surface, and from the same cause joined with the action of the moon on the equatorial protuberance there results the precession of the equinoxes. Thus the multiplication of effects is obvious. Several of the differentiations due to the gradual cooling of the Earth have been already noticed —as the formation of a crust, the solidification of sublimed elements, the precipitation of water, &c.,—and we here again refer to them merely to point out that they are simultaneous effects of the one cause, diminishing heat. Let us now, however, observe the multiplied changes afterwards arising from the continuance of this one cause. The cooling of the Earth involves its contraction. Hence the solid crust first formed is presently too large for the shrinking nucleus; and as it cannot support itself, inevitably follows the nucleus. But a spheroidal envelope cannot sink down into contact with a smaller internal spheroid, without disruption: it must run into wrinkles as the rind of an apple does when the bulk of its interior decreases from evaporation. As the cooling progresses and the envelope thickens, the ridges consequent on these contractions will become greater, rising ultimately into hills and mountains; and the later systems of mountains thus produced will not only be higher, as we find them to be, but will be longer, as we also find them to be. Thus, leaving out of view other modifying forces, we see what immense heterogeneity of surface has arisen from the one cause, loss of heat—a heterogeneity which the telescope shows us to be paralleled on the face of Mars, and which in the moon too, where aqueous and atmospheric agencies have been absent, it reveals under a somewhat different form. But we have yet to notice another kind of heterogeneity of surface similarly and simultaneously caused. While the Earth's crust was still thin, the ridges produced by its contraction must not only have been small, but the spaces between these ridges must have rested with great evenness upon the subjacent liquid spheroid; and the water in those arctic and antarctic regions in which it first condensed, must have been evenly distributed. But as fast as the crust thickened and gained corresponding strength, the lines of fracture from time to time caused in it, must have occurred at greater distances apart; the intermediate surfaces must have followed the contracting nucleus with less uniformity; and there must have resulted larger areas of land and water. If any one, after wrapping up an orange in tissue paper, and observing not only how small are the wrinkles, but how evenly the intervening spaces lie upon the surface of the orange, will then wrap it up in thick cartridge-paper, and note both the greater height of the ridges and the larger spaces throughout which the paper does not touch the orange, he will realize the fact that, as the Earth's solid envelope grew thicker, the areas of elevation and depression increased. In place of islands homogeneously dispersed amid an all-embracing sea, there must have gradually arisen heterogeneous arrangements of continent and ocean. Once more, this double change in the extent and in the elevation of the lands, involved yet another species of heterogeneity—that of coast-line. A tolerably even surface raised out of the ocean must have a simple, regular sea-margin; but a surface varied by table-lands and intersected by mountain-chains must, when raised out of the ocean, have an outline extremely irregular both in its leading features and in its details. Thus, multitudinous geological and geographical results are slowly brought about by this one cause—the contraction of the Earth.

When we pass from the agency termed igneous, to aqueous and atmospheric agencies, we see the like ever-growing complications of effects. The denuding actions of air and water, joined with those of changing temperature, have, from the beginning, been modifying every exposed surface. Oxidation, heat, wind, frost, rain, glaciers, rivers, tides, waves, have been unceasingly producing disintegration; varying in kind and amount according to local circumstances. Acting upon a tract of granite, they here work scarcely an appreciable effect; there cause exfoliations of the surface, and a resulting heap of *débris* and boulders; and elsewhere, after decomposing the feldspar into a white clay, carry away this and the accompanying quartz and mica, and deposit them in separate beds, fluviatile and marine. When the exposed land consists of several unlike kinds of sedimentary strata, or igneous rocks, or both, denudation produces changes proportionably more heterogeneous. The formations being disintegrable in different degrees, there follows an increased irregularity of surface. The areas drained by different rivers being differently constituted, these rivers carry down to the sea different combinations of ingredients; and so sundry new strata of unlike compositions are formed. And here we may see very simply illustrated, the truth, which we shall presently have to trace out in more involved cases, that in proportion to the heterogeneity of the object or objects on which any force expends itself, is the heterogeneity of the effects. A continent of complex structure, exposing many strata irregularly distributed, raised to various levels, tilted up at all angles, will, under the same denuding agencies, give origin to innumerable and involved results: each district must be differently modified; each river must carry down a different kind of detritus; each deposit must be differently distributed by the entangled currents, tidal and other, which wash the contorted shores; and this multiplication of results must manifestly be greatest where the complexity of surface is greatest.

Here we might show how the general truth, that every active force produces more than one change, is again exemplified in the highly-involved flow of the tides, in the ocean currents, in the winds, in the distribution of rain, in the distribution of heat, and so forth. But not to dwell upon these, let us, for the fuller elucidation of this truth in relation to the inorganic world, consider what would be the consequences of some extensive cosmical catastrophe—say the subsidence of Central America. The immediate results of the disturbance would themselves be sufficiently complex. Besides the numberless dislocations of strata, the ejections of igneous matter, the propagation of earthquake vibrations thousands of miles around, the loud explosions, and the escape of gases; there would be the rush of the Atlantic and Pacific Oceans to fill the vacant space, the subsequent recoil of enormous waves, which would traverse both these oceans and produce myriads of changes along their shores, the corresponding atmospheric waves complicated by the currents surrounding each volcanic vent, and the electrical discharges with which such disturbances are accompanied. But these temporary effects would be insignificant compared with the permanent ones. The currents of the Atlantic and Pacific would be altered in their directions and amounts. The distribution of heat achieved by these ocean currents would be different from what it is. The arrangement of the isothermal lines, not only on neighbouring continents, but even throughout Europe, would be changed. The tides would flow differently from what they do now. There would be more or less modification of the winds in their periods, strengths, directions, qualities. Rain would fall scarcely anywhere at the same times and in the same quantities as at present. In short, the meteorological conditions thousands of miles off, on all sides, would be more or less revolutionized. Thus, without taking into account the infinitude of modifications which these changes would produce upon the flora and fauna, both of land and sea, the reader will perceive the immense heterogeneity of the results wrought out by one force, when that force expends itself upon a previously complicated area; and he will draw the corollary that from the beginning the complication has advanced at an increasing rate.

Before going on to show how organic progress also depends on the law that every force produces more than one change, we have to notice the manifestation of this law in yet another species of inorganic progress—namely, chemical. The same general causes that have wrought out the heterogeneity of the Earth, physically considered, have simultaneously wrought out its chemical heterogeneity. There is every reason to believe that at an extreme heat the elements cannot combine. Even under such heat as can be artificially produced, some very strong affinities yield, as, for instance, that of oxygen for hydrogen; and the great majority of chemical compounds are decomposed at much lower temperatures. But without insisting on the highly probable inference, that when the Earth was in its first state of incandescence there were no chemical combinations at all, it will suffice for our purpose to point to the unquestionable fact that the compounds which can exist at the highest temperatures, and which must, therefore, have been the first that were formed as the Earth cooled, are those of the simplest constitutions. The protoxides—including under that head the alkalies, earths, &c.—are, as a class, the most stable compounds we know: most of them resisting decomposition by any heat we can generate. These are combinations of the simplest order—are but one degree less homogeneous than the elements themselves. More heterogeneous, less stable, and therefore later in the Earth's history, are the deutoxides, tritoxides, peroxides, &c.; in which two, three, four, or more atoms of oxygen are united with one atom of metal or other element. Higher than these in heterogeneity are the hydrates; in which an oxide of hydrogen, united with an oxide of some other element, forms a substance whose atoms severally contain at least four ultimate atoms of three different kinds. Yet more heterogeneous and less stable still are the salts; which present us with molecules each made up of five, six, seven, eight, ten, twelve, or more atoms, of three, if not more, kinds. Then there are the hydrated salts, of a yet greater heterogeneity, which undergo partial decomposition at much lower temperatures. After them come the further complicated supersalts and double salts, having a stability again decreased; and so throughout. Without entering into qualifications for which space fails, we believe no chemist will deny it to be a general law of these inorganic combinations that, *other things equal*, the stability decreases as the complexity increases. When we pass to the compounds of organic chemistry, we find this general law still further exemplified: we find much greater complexity and much less stability. A molecule of albumen, for instance, consists of 482 ultimate atoms of five different kinds. Fibrine, still more intricate in constitution, contains in each molecule, 298 atoms of carbon, 49 of nitrogen, 2 of sulphur, 228 of hydrogen, and 92 of oxygen—in all, 669 atoms; or, more strictly speaking, equivalents. And these two substances are so unstable as to decompose at quite ordinary temperatures; as that to which the outside of a joint of roast meat is exposed. Thus it is manifest that the present chemical heterogeneity of the Earth's surface has arisen by degrees, as the decrease of heat has permitted; and that it has shown itself in three forms—first, in the multiplication of chemical compounds; second, in the greater number of different elements contained in the more modern of these compounds; and third, in the higher and more varied multiples in which these more numerous elements combine.

To say that this advance in chemical heterogeneity is due to the one cause, diminution of the Earth's temperature, would be to say too much; for it is clear that aqueous and atmospheric agencies have been concerned; and further, that the affinities of the elements themselves are implied. The cause has all along been a composite one: the cooling of the Earth having been simply the most general of the concurrent causes, or assemblage of conditions. And here, indeed, it may be remarked that in the several classes of facts already dealt with (excepting, perhaps, the first), and still more in those with which we shall presently deal, the causes are more or less compound; as indeed are nearly all causes with which we are acquainted. Scarcely any change can rightly be ascribed to one agency alone, to the neglect of the permanent or temporary conditions under which only this agency produces the change. But as it does not materially affect our argument, we prefer, for simplicity's sake, to use throughout the popular mode of expression. Perhaps it will be further objected, that to assign loss of heat as the cause of any changes, is to attribute these changes not to a force, but to the absence of a force. And this is true. Strictly speaking, the changes should be attributed to those forces which come into action when the antagonist force is withdrawn. But though there is inaccuracy in saying that the freezing of water is due to the loss of its heat, no practical error arises from it; nor will a parallel laxity of expression vitiate our statements respecting the multiplication of effects. Indeed, the objection serves but to draw attention to the fact, that not only does the exertion of a force produce more than one change, but the withdrawal of a force produces more than one change.

Returning to the thread of our exposition, we have next to trace, throughout organic progress, this same all-pervading principle. And here, where the evolution of the homogeneous into the heterogeneous was first observed, the production of many effects by one cause is least easy to demonstrate. The development of a seed into a plant, or an ovum into an animal, is so gradual, while the forces which determine it are so involved, and at the same time so unobtrusive, that it is difficult to detect the multiplication of effects which is elsewhere so obvious. But, guided by indirect evidence, we may safely conclude that here too the law holds. Note, first, how numerous are the changes which any marked action works upon an adult organism—a human being, for instance. An alarming sound or sight, besides the impressions on the organs of sense and the nerves, may produce a start, a scream, a distortion of the face, a trembling consequent on general muscular relaxation, a burst of perspiration, a rush of blood to the brain, followed possibly by arrest of the heart's action and by syncope; and if the subject be feeble, an indisposition with its long train of complicated symptoms may set in. Similarly in cases of disease. A minute portion of the small-pox virus introduced into the system, will, in a severe case, cause, during the first stage, rigors, heat of skin, accelerated pulse, furred tongue, loss of appetite, thirst, epigastric uneasiness, vomiting, headache, pains in the back and limbs, muscular weakness, convulsions, delirium, &c.; in the second stage, cutaneous eruption, itching, tingling, sore throat, swelled fauces, salivation, cough, hoarseness, dyspnœa, &c.; and in the third stage, œdematous inflammations, pneumonia, pleurisy, diarrhœa, inflammation of the brain, ophthalmia, erysipelas, &c.: each of which enumerated symptoms is itself more or less complex. Medicines, special foods, better air, might in like manner be instanced as producing multipled results. Now it needs only to consider that the many changes thus wrought by one force upon an adult organism, will be in part paralleled in an embryo organism, to understand how here also, the evolution of the homogeneous into the heterogeneous may be due to the production of many effects by one cause. The external heat, which, falling on a matter having special proclivities, determines the first complications of the germ, may, by acting on these, superinduce further complications; upon these still higher and more numerous ones; and so on continually: each organ as it is developed serving, by its actions and reactions on the rest, to initiate new complexities. The first pulsations of the fœtal heart must simultaneously aid the unfolding of every part. The growth of each tissue, by taking from the blood special proportions of elements, must modify the constitution of the blood; and so must modify the nutrition of all the other tissues. The heart's action, implying as it does a certain waste, necessitates an addition to the blood of effete matters, which must influence the rest of the system, and perhaps, as some think, cause the formation of excretory organs. The nervous connexions established among the viscera must further multiply their mutual influences; and so continually. Still stronger becomes the probability of this view when we call to mind the fact, that the same germ may be evolved into different forms according to circumstances. Thus, during its earlier stages, every embryo is sexless—becomes either male or female as the balance of forces acting on it determines. Again, it is a well-established fact that the larva of a working-bee will develop into a queen-bee, if before it is too late, its food be changed to that on which the larvæ of queen-bees are fed. All which instances suggest that the proximate cause of each advance in embryonic complication is the action of incident forces upon the complication previously existing. Indeed, we may find *a priori* reason to think that the evolution proceeds after this manner. For since no germ, animal or vegetal, contains the slightest rudiment or indication of the future organism—since the microscope has shown us that the first process set up in every fertilized germ, is a process of repeated spontaneous fissions ending in the production of a mass of cells, not one of which exhibits any special character; there seems no alternative but to suppose that the partial organization at any moment existing in a growing embryo, is transformed by the agencies acting upon it into the succeeding phase of organization, and this into the next, until, through ever-increasing complexities, the ultimate form is reached. Not indeed that we can thus really explain the production of any plant or animal. We are still in the dark respecting those mysterious properties in virtue of which the germ, when subject to fit influences, undergoes the special changes that begin the series of transformations. All we aim to show, is, that given a germ possessing those particular proclivities distinguishing the species to which it belongs, and the evolution of an organism from it, probably depends on that multiplication of effects which we have seen to be the cause of progress in general, so far as we have yet traced it.

When, leaving the development of single plants and animals, we pass to that of the Earth's flora and fauna, the course of our argument again becomes clear and simple. Though, as was admitted in the first part of this article, the fragmentary facts Paleontology has accumulated, do not clearly warrant us in saying that, in the lapse of geologic time, there have been evolved more heterogeneous organisms, and more heterogeneous assemblages of organisms, yet we shall now see that there *must* ever have been a tendency towards these results. We shall find that the production of many effects by one cause, which as already shown, has been all along increasing the physical heterogeneity of the Earth, has further involved an increasing heterogeneity in its flora and fauna, individually and collectively. An illustration will make this clear. Suppose that by a series of upheavals, occurring, as they are now known to do, at long intervals, the East Indian Archipelago were to be, step by step, raised into a continent, and a chain of mountains formed along the axis of elevation. By the first of these upheavals, the plants and animals inhabiting Borneo, Sumatra, New Guinea, and the rest, would be subjected to slightly modified sets of conditions. The climate in general would be altered in temperature, in humidity, and in its periodical variations; while the local differences would be multiplied. These modifications would affect, perhaps inappreciably, the entire flora and fauna of the region. The change of level would produce additional modifications: varying in different species, and also in different members of the same species, according to their distance from the axis of elevation. Plants, growing only on the sea-shore in special localities, might become extinct.

Others, living only in swamps of a certain humidity, would, if they survived at all, probably undergo visible changes of appearance. While still greater alterations would occur in the plants gradually spreading over the lands newly raised above the sea. The animals and insects living on these modified plants, would themselves be in some degree modified by change of food, as well as by change of climate; and the modification would be more marked where, from the dwindling or disappearance of one kind of plant, an allied kind was eaten. In the lapse of the many generations arising before the next upheaval, the sensible or insensible alterations thus produced in each species would become organized—there would be a more or less complete adaptation to the new conditions. The next upheaval would superinduce further organic changes, implying wider divergences from the primary forms; and so repeatedly. But now let it be observed that the revolution thus resulting would not be a substitution of a thousand more or less modified species for the thousand original species; but in place of the thousand original species there would arise several thousand species, or varieties, or changed forms. Each species being distributed over an area of some extent, and tending continually to colonize the new area exposed, its different members would be subject to different sets of changes. Plants and animals spreading towards the equator would not be affected in the same way as others spreading from it. Those spreading towards the new shores would undergo changes unlike the changes undergone by those spreading into the mountains. Thus, each original race of organisms, would become the root from which diverged several races differing more or less from it and from each other; and while some of these might subsequently disappear, probably more than one would survive in the next geologic period: the very dispersion itself increasing the chances of survival. Not only would there be certain modifications thus caused by change of physical conditions and food, but also in some cases other modifications caused by change of habit. The fauna of each island, peopling, step by step, the newly-raised tracts, would eventually come in contact with the faunas of other islands; and some members of these other faunas would be unlike any creatures before seen. Herbivores meeting with new beasts of prey, would, in some cases, be led into modes of defence or escape differing from those previously used; and simultaneously the beasts of prey would modify their modes of pursuit and attack. We know that when circumstances demand it, such changes of habit *do* take place in animals; and we know that if the new habits become the dominant ones, they must eventually in some degree alter the organization. Observe now, however, a further consequence. There must arise not simply a tendency towards the differentiation of each race of organisms into several races; but also a tendency to the occasional production of a somewhat higher organism. Taken in the mass these divergent varieties which have been caused by fresh physical conditions and habits of life, will exhibit changes quite indefinite in kind and degree; and changes that do not necessarily constitute an advance. Probably in most cases the modified type will be neither more nor less heterogeneous than the original one. In some cases the habits of life adopted being simpler than before, a less heterogeneous structure will result: there will be a retrogradation. But it *must* now and then occur, that some division of a species, falling into circumstances which give it rather more complex experiences, and demand actions somewhat more involved, will have certain of its organs further differentiated in proportionately small degrees,—will become slightly more heterogeneous. Thus, in the natural course of things, there will from time to time arise an increased heterogeneity both of the Earth's flora and fauna, and of individual races included in them. Omitting detailed explanations, and allowing for the qualifications which cannot here be specified, we think it is clear that geological mutations have all along tended to complicate the forms of life, whether regarded separately or collectively. The same causes which have led to the evolution of the Earth's crust from the simple into the complex, have simultaneously led to a parallel evolution of the Life upon its surface. In this case, as in previous ones, we see that the transformation of the homogeneous into the heterogeneous is consequent upon the universal principle, that every active force produces more than one change.

The deduction here drawn from the established truths of geology and the general laws of life, gains immensely in weight on finding it to be in harmony with an induction drawn from direct experience. Just that divergence of many races from one race, which we inferred must have been continually occurring during geologic time, we know to have occurred during the pre-historic and historic periods, in man and domestic animals. And just that multiplication of effects which we concluded must have produced the first, we see has produced the last. Single causes, as famine, pressure of population, war, have periodically led to further dispersions of mankind and of dependent creatures: each such dispersion initiating new modifications, new varieties of type. Whether all the human races be or be not derived from one stock, philology makes it clear that whole groups of races now easily distinguishable from each other, were originally one race,—that the diffusion of one race into different climates and conditions of existence, has produced many modified forms of it. Similarly with domestic animals. Though in some cases—as that of dogs—community of origin will perhaps be disputed, yet in other cases—as that of the sheep or the cattle of our own country—it will not be questioned that local differences of climate, food, and treatment, have transformed one original breed into numerous breeds now become so far distinct as to produce unstable hybrids. Moreover, through the complication of effects flowing from single causes, we here find, what we before inferred, not only an increase of general heterogeneity, but also of special heterogeneity. While of the divergent divisions and subdivisions of the human race many have undergone changes not constituting an advance; while in some the type may have degraded; in others it has become decidedly more heterogeneous. The civilized European departs more widely from the vertebrate archetype than does the savage. Thus, both the law and the cause of progress, which, from lack of evidence, can be but hypothetically substantiated in respect of the earlier forms of life on our globe, can be actually substantiated in respect of the latest forms.*

If the advance of Man towards greater heterogeneity is traceable to the production of many effects by one cause, still more clearly may the advance of Society towards greater heterogeneity be so explained. Consider the growth of an industrial organization. When, as must occasionally happen, some member of a tribe displays unusual aptitude for making an article of general use—a weapon, for instance—which was before made by each man for himself, there arises a tendency towards the differentiation of that member into a maker of such weapon. His companions—warriors and hunters all of them,—severally feel the importance of having the best weapons that can be made; and are therefore certain to offer strong inducements to this skilled individual to make weapons for them. He, on the other hand, having not only an unusual faculty, but an unusual liking, for making such weapons (the talent and the desire for any occupation being commonly associated), is predisposed to fulfil each commission on the offer of an adequate reward: especially as his love of distinction is also gratified and his living facilitated. This first specialization of function, once commenced, tends ever to become more decided. On the side of the weapon-maker practice gives increased skill—increased superiority to his products. On the side of his clients, cessation of practice entails decreased skill. Thus the influences which determine this division of labour grow stronger in both ways; and the incipient heterogeneity is, on the average of cases, likely to become permanent for that generation if no longer. This process not only differentiates the social mass into two parts, the one monopolizing, or almost monopolizing, the performance of a certain function, and the other losing the habit, and in some measure the power, of performing that function; but it tends to initiate other differentiations. The advance described implies the introduction of barter,—the maker of weapons has, on each occasion, tô be paid in such other articles as he agrees to take in exchange. He will not habitually take in exchange one kind of article, but many kinds. He does not want mats only, or skins, or fishing-gear, but he wants all these, and on each occasion will bargain for the particular things he most needs. What follows? If among his fellows there exist any slight differences of skill in the manufacture of these various things, as there are almost sure to do, the weapon-maker will take from each one the thing which that one excels in making: he will exchange for mats with him whose mats are superior, and will bargain for the fishing-gear of him who has the best. But he who has bartered away his mats or his fishing-gear, must make other mats or fishing-gear for himself; and in so doing must, in some degree, further develop his aptitude. Thus it results that the small specialities of faculty possessed by various members of the tribe, will tend to grow more decided. And whether or not there ensue distinct differentiations of other individuals into makers of particular articles, it is clear that incipient differentiations take place throughout the tribe: the one original cause produces not only the first dual effect, but a number of secondary dual effects, like in kind, but minor in degree. This process, of which traces may be seen among schoolboys, cannot well produce lasting effects in an unsettled tribe; but where there grows up a fixed and multiplying community, such differentiations become permanent, and increase with each generation. The enhanced demand for every commodity, intensifies the functional activity of each specialized person or class; and this renders the specialization more definite where it already exists, and establishes it where it is but nascent. By increasing the pressure on the means of subsistence, a larger population again augments these results; seeing that each person is forced more and more to confine himself to that which he can do best, and by which he can gain most. Presently, under these same stimuli, new occupations arise. Competing workers, ever aiming to produce improved articles, occasionally discover better processes or raw materials. The substitution of bronze for stone entails on him who first makes it a great increase of demand; so that he or his successor eventually finds all his time occupied in making the bronze for the articles he sells, and is obliged to depute the fashioning of these articles to others; and, eventually, the making of bronze, thus differentiated from a pre-existing occupation, becomes an occupation by itself. But now mark the ramified changes which follow this change. Bronze presently replaces stone, not only in the articles it was first used for, but in many others—in arms, tools, and utensils of various kinds: and so affects the manufacture of them. Further, it affects the processes which these utensils subserve, and the resulting products, —modifies buildings, carvings, personal decorations. Yet again, it sets going manufactures which were before impossible, from lack of a material fit for the requisite implements. And all these changes react on the people—increase their manipulative skill, their intelligence, their comfort,—refine their habits and tastes. Thus the evolution of a homogeneous society into a heterogeneous one, is clearly consequent on the general principle, that many effects are produced by one cause.

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* The argument concerning organic evolution contained in this paragraph and the one preceding it, stands verbatim as it did when first published in the *Westminster Review* for April, 1857. I have thus left it without the alteration of a word that it may show the view I then held concerning the origin of species. The sole cause recognized is that of direct adaptation of constitution to conditions consequent on inheritance of the modifications of structure resulting from use and disuse. There is no recognition of that further cause disclosed in Mr. Darwin's work, published two and a half years later—the indirect adaptation resulting from the natural selection of favourable variations. The multiplication of effects is, however, equally illustrated in whatever way the adaptation to changing conditions is effected, or if it is effected in both ways, as I hold. I may add that there is indicated the view that the succession of organic forms is not serial but proceeds by perpetual divergence and re-divergence—that there has been a continual "divergence of many races from one race": each species being a "root" from which several other species branch out; and the growth of a tree being thus the implied symbol.

Space permitting, we might show how the localization of special industries in special parts of a kingdom, as well as the minute subdivision of labour in the making of each commodity, are similarly determined. Or, turning to a somewhat different order of illustrations, we might dwell on the multitudinous changes—material, intellectual, moral, —caused by printing; or the further extensive series of changes wrought by gunpowder. But leaving the intermediate phases of social development, let us take a few illustrations from its most recent and its passing phases. To trace the effects of steam-power, in its manifold applications to mining, navigation, and manufactures of all kinds, would carry us into unmanageable detail. Let us confine ourselves to the latest embodiment of steam power—the locomotive engine. This, as the proximate cause of our railway system, has changed the face of the country, the course of trade, and the habits of the people. Consider, first, the complicated sets of changes that precede the making of every railway—the provisional arrangements, the meetings, the registration, the trial section, the parliamentary survey, the lithographed plans, the books of reference, the local deposits and notices, the application to

Parliament, the passing Standing Orders Committee, the first, second, and third readings: each of which brief heads indicates a multiplicity of transactions, and the extra development of sundry occupations—as those of engineers, surveyors, lithographers, parliamentary agents, share-brokers; and the creation of sundry others—as those of traffic-takers, reference-takers. Consider, next, the yet more marked changes implied in railway construction—the cuttings, embankings, tunnellings, diversions of roads; the building of bridges and stations, the laying down of ballast, sleepers, and rails; the making of engines, tenders, carriages, and waggons: which processes, acting on numerous trades, increase the importation of timber, the quarrying of stone, the manufacture of iron, the mining of coal, the burning of bricks; institute a variety of special manufactures weekly advertised in the *Railway Times;* and, finally, open the way to sundry new occupations, as those of drivers, stokers, cleaners, plate-layers, &c., &c. And then consider the changes, still more numerous and involved, which railways in action produce on the community at large. Business agencies are established where previously they would not have paid; goods are obtained from remote wholesale houses instead of near retail ones; and commodities are used which distance once rendered inaccessible. Again, the diminished cost of carriage tends to specialize more than ever the industries of different districts—to confine each manufacture to the parts in which, from local advantages, it can be best carried on. Further, the fall in freights, facilitating distribution, equalizes prices, and also, on the average, lowers prices: thus bringing divers articles within the means of those before unable to buy them, and so increasing their comforts and improving their habits. At the same time the practice of travelling is immensely extended. People who never before dreamed of it, take trips to the sea; visit their distant relations; make tours; and so we are benefited in body, feelings, and ideas. The more prompt transmission of letters and of news produces other marked changes—makes the pulse of the nation faster. Once more, there arises a wide dissemination of cheap literature through railway book-stalls, and of advertisements in railway carriages: both of them aiding ulterior progress. And the countless changes here briefly indicated are consequent on the invention of the locomotive engine. The social organism has been rendered more heterogeneous in virtue of the many new occupations introduced, and the many old ones further specialized; prices of nearly all things in every place have been altered; each trader has modified his way of doing business; and every person has been affected in his actions, thoughts, emotions.

Illustrations to the same effect might be indefinitely accumulated, but they are needless. The only further fact demanding notice, is, that we here see still more clearly the truth before pointed out, that in proportion as the area on which any force expends itself becomes heterogeneous, the results are in a yet higher degree multiplied in number and kind. While among the simple tribes to whom it was first known, caoutchouc caused but few changes, among ourselves the changes have been so many and varied that the history of them occupies a volume.* Upon the small, homogeneous community inhabiting one of the Hebrides, the electric telegraph would produce, were it used, scarcely any results; but in England the results it produces are multitudinous. The comparatively simple organization under which our ancestors lived five centuries ago, could have undergone but few modifications from an event like the recent one at Canton; but now, the legislative decision respecting it sets up many hundreds of complex modifications, each of which will be the parent of numerous future ones.

Space permitting, we could willingly have pursued the argument in relation to all the subtler results of civilization. As before we showed that the law of progress to which the organic and inorganic worlds conform, is also conformed to by Language, the plastic arts, Music, &c.; so might we here show that the cause which we have hitherto found to determine progress holds in these cases also. Instances might be given proving how, in Science, an advance of one division presently advances other divisions—how Astronomy has been immensely forwarded by discoveries in Optics, while other optical discoveries have initiated Microscopic Anatomy, and greatly aided the growth of Physiology—how Chemistry has indirectly increased our knowledge of Electricity, Magnetism, Biology, Geology—how Electricity has reacted on Chemistry and Magnetism, and has developed our views of Light and Heat. In Literature the same truth might be exhibited in the manifold effects of the primitive mystery-play, as originating the modern drama, which has variously branched; or in the still multiplying forms of periodical literature which have descended from the first newspaper, and which have severally acted and reacted on other forms of literature and on each other. The influence which a new school of Painting—as that of the pre-Raffaelites—exercises upon other schools; the hints which all kinds of pictorial art are deriving from Photography; the complex results of new critical doctrines, as those of Mr. Ruskin, might severally be dwelt upon as displaying the like multiplication of effects.

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* "Personal Narrative of the Origin of the Caoutchouc, or India-Rubber Manufacture in England." By Thomas Hancock.

But we venture to think our case is already made out. The imperfections of statement which brevity has necessitated, do not, we believe, invalidate the propositions laid down. The qualifications here and there demanded would not, if made, affect the inferences. Though, in tracing the genesis of progress, we have frequently spoken of complex causes as if they were simple ones; it still remains true that such causes are far less complex than their results. Detailed criticisms do not affect our main position. Endless facts go to show that every kind of progress is from the homogeneous to the heterogeneous; and that it is so because each change is followed by many changes. And it is significant that where the facts are most accessible and abundant, there these truths are most manifest.

However, to avoid committing ourselves to more than is yet proved, we must be content with saying that such are the law and the cause of all progress that is known to us. Should the Nebular Hypothesis ever be established, then it will become manifest that the Universe at large, like every organism, was once homogeneous; that as a whole, and in every detail, it has unceasingly advanced towards greater heterogeneity. It will be seen that as in each event of to-day, so from the beginning, the decomposition of every expended force into several forces has been perpetually producing a higher complication; that the increase of heterogeneity so brought about is still going on and must continue to go on; and that thus progress is not an accident, not a thing within human control, but a beneficent necessity.

A few words must be added on the ontological bearings of our argument. Probably not a few will conclude that here is an attempted solution of the great questions with which Philosophy in all ages has perplexed itself. Let none thus deceive themselves. After all that has been said, the ultimate mystery remains just as it was. The explanation of that which is explicable, does but bring out into greater clearness the inexplicableness of that which remains behind. Little as it seems to do so, fearless inquiry tends continually to give a firmer basis to all true Religion. The timid sectarian, obliged to abandon one by one the superstitions bequeathed to him, and daily finding his cherished beliefs more and more shaken, secretly fears that all things may some day be explained; and has a corresponding dread of Science: thus evincing the profoundest of all infidelity—the fear lest the truth be bad. On the other hand, the sincere man of science, content to follow wherever the evidence leads him, becomes by each new inquiry more profoundly convinced that the Universe is an insoluble problem. Alike in the external and the internal worlds, he sees himself in the midst of ceaseless changes, of which he can discover neither beginning nor end. If, tracing back the evolution of things, he allows himself to entertain the hypothesis that all matter once existed in a diffused form, he finds it impossible to conceive how this came to be so; and equally, if he speculates on the future, he can assign no limit to the grand succession of phenomena ever unfolding themselves before him. Similarly, if he looks inward, he perceives that both terminations of the thread of consciousness are beyond his grasp: he cannot remember when or how consciousness commenced, and he cannot examine the consciousness at any moment existing; for only a state of consciousness which is already past can become the object of thought, and never one which is passing. When, again, he turns from the succession of phenomena, external or internal, to their essential nature, he is equally at fault. Though he may succeed in resolving all properties of objects into manifestations of force, he is not thereby enabled to conceive what force is; but finds, on the contrary, that the more he thinks about it, the more he is baffled. Similarly, though analysis of mental actions may finally bring him down to sensations as the original materials out of which all thought is woven, he is none the forwarder; for he cannot in the least comprehend sensation. Inward and outward things he thus discovers to be alike inscrutable in their ultimate genesis and nature. He sees that the Materialist and Spiritualist controversy is a mere war of words; the disputants being equally absurd—each believing he understands that which it is impossible for any man to understand. In all directions his investigations eventually bring him face to face with the unknowable; and he ever more clearly perceives it to be the unknowable. He learns at once the greatness and the littleness of human intellect—its power in dealing with all that comes within the range of experience; its impotence in dealing with all that transcends experience. He feels more vividly than any others can feel, the utter incomprehensibleness of the simplest fact, considered in itself. He alone truly *sees* that absolute knowledge is impossible. He alone *knows* that under all things there lies an impenetrable mystery.

# TRANSCENDENTAL PHYSIOLOGY.

[*First published in* The National Review *for October*, 1857, *under the title of* "*The Ultimate Laws of Physiology*". *The title* "*Transcendental Physiology*", *which the editor did not approve, was restored when the essay was re-published with others in* 1857.]

THE title Transcendental Anatomy is used to distinguish that division of biological science which treats, not of the structures of individual organisms considered separately, but of the general principles of structure common to vast and varied groups of organisms,—the unity of plan discernible throughout multitudinous species, genera, and orders, which differ widely in appearance. And here, under the head of Transcendental Physiology, we purpose putting together sundry laws of development and function which hold not of particular kinds or classes of organisms, but of all organisms: laws, some of which have not, we believe, been hitherto enunciated.

By way of unobtrusively introducing the general reader to biological truths of this class, let us begin by noticing one or two with which he is familiar. Take first, the relation between the activity of an organ and its growth. This is a universal relation. It holds, not only of a bone, a muscle, a nerve, an organ of sense, a mental faculty; but of every gland, every viscus, every element of the body. It is seen, not in man only, but in each animal which affords us adequate opportunity of tracing it. Always providing that the performance of function is not so excessive as to produce disorder, or to exceed the repairing powers either of the system at large or of the particular agencies by which nutriment is brought to the organ,—always providing this, it is a law of organized bodies that, other things equal, development varies as function. On this law are based all maxims and methods of right education, intellectual, moral, and physical; and when statesmen are wise enough to see it, this law will be found to underlie all right legislation.

Another truth co-extensive with the organic world, is that of hereditary transmission. It is not, as commonly supposed, that hereditary transmission is exemplified merely in re-appearance of the family peculiarities displayed by immediate or remote progenitors. Nor does the law of hereditary transmission comprehend only such more general facts as that modified plants or animals become the parents of permanent varieties; and that new kinds of potatoes, new breeds of sheep, new races of men, have been thus originated. These are but minor exemplifications of the law. Understood in its entirety, the law is that each plant or animal produces others of like kind with itself: the likeness of kind consisting not so much in the repetition of individual traits as in the assumption of the same general structure. This truth has been made by daily illustration so familiar as nearly to have lost its significance. That wheat produces wheat,—that existing oxen are descended from ancestral oxen,—that every unfolding organism ultimately takes the form of the class, order, genus, and species from which it sprang; is a fact which, by force of repetition, has assumed in our minds the character of a necessity. It is in this, however, that the law of hereditary transmission is principally displayed; the phenomena commonly named as exemplifying it being quite subordinate manifestations. And the law, as thus understood, is universal. Not forgetting the apparent, but only apparent, exceptions presented by the strange class of phenomena known as "alternate generation," the truth that like produces like is common to all types of organisms.

Let us take next a universal physiological law of a less conspicuous kind. To the ordinary observer, it seems that the multiplication of organisms proceeds in various ways. He sees that the young of the higher animals when born resemble their parents; that birds lay eggs, which they foster and hatch; that fish deposit spawn and leave it. Among plants, he finds that while in some cases new individuals grow from seeds only, in other cases they also grow from tubers; that by certain plants layers are sent out, take root, and develop new individuals; and that many plants can be reproduced from cuttings. Further, in the mould that quickly covers stale food, and the infusoria that soon swarm in water exposed to air and light, he sees a mode of generation which, seeming inexplicable, he is apt to consider "spontaneous." The reader of popular science thinks the modes of reproduction still more various. He learns that whole tribes of creatures multiply by gemmation—by a development from the body of the parent of buds which, after unfolding into the parental form, separate and lead independent lives. Concerning microscopic forms of both animal and vegetal life, he reads that the ordinary mode of multiplication is by spontaneous fission—a splitting up of the original individual into two or more individuals, which by and by severally repeat the process. Still more remarkable are the cases in which, as in the *Aphis*, an egg gives rise to an imperfect female, from which other imperfect females are born viviparously, grow, and in their turns bear other imperfect females; and so on for eight, ten, or more generations, until finally, perfect males and females are viviparously produced. But now under all these, and many more, modified modes of multiplication, the physiologist finds complete uniformity. The starting-point, not only of every higher animal or plant, but of every clan of organisms which by fission or gemmation have sprung from a single organism, is always a spore, seed, or ovum. The millions of infusoria or of aphides which, by sub-division or gemmation, have proceeded from one individual; the countless plants which have been successively propagated from one original plant by cuttings or tubers; are, in common with the highest creature, primarily descended from a fertilized germ. And in all cases—in the humblest alga as in the oak, in the protozoon as in the mammal—this fertilized germ results from the union of the contents of two cells. Whether, as among the lowest forms of life, these two cells are seemingly identical in nature; or whether, as among higher forms, they are distinguishable into sperm-cell and germ-cell; it remains throughout true that from their combination results the mass out of which is evolved a new organism or new series of organisms. That this law is without exception we are not prepared to say; for in the case of the *Aphis* certain experiments are thought to imply that under special conditions the descendants of an original individual may continue multiplying for ever, without further fecundation. But we know of no case where it *actually is* so; for although there are certain plants of which the seeds have never been seen, it is more probable that our observations are in fault than that these plants are exceptions. And until we find undoubted exceptions, the above-stated induction must stand. Here, then, we have another of the truths of Transcendental Physiology: a truth which, so far as we know, *transcends* all distinctions of genus, order, class, kingdom, and applies to every living thing.

Yet another generalization of like universality expresses the process of organic development. To the ordinary observer there seems no unity in this. No obvious parallelism exists between the unfolding of a plant and the unfolding of an animal. There is no manifest similarity between the development of a mammal, which proceeds without break from its first to its last stage, and that of an insect, which is divided into strongly-marked stages—egg, larva, pupa, imago. Nevertheless it is now an established fact, that all organisms are evolved after one general method. At the outset the germ of every plant or animal is relatively homogeneous; and advance towards maturity is advance towards greater heterogeneity. Each organized thing commences as an almost structureless mass, and reaches its ultimate complexity by the establishment of distinctions upon distinctions,—by the divergence of tissues from tissues and organs from organs. Here, then, we have yet another biological law of transcendent generality.

Having thus recognized the scope of Transcendental Physiology as presented in its leading truths, we are prepared for the considerations that are to follow.

And first, returning to the last of the great generalizations above given, let us inquire more nearly how this change from the homogeneous to the heterogeneous is carried on. Usually it is said to result from successive differentiations. This, however, cannot be considered a complete account of the process. During the evolution of an organism there occur, not only separations of parts, but coalescences of parts. There is not only segregation, but aggregation. The heart, at first a simple pulsating blood-vessel, by and by twists upon itself and becomes integrated. The bile-cells constituting the rudimentary liver, do not merely diverge from the surface of the intestine in which they at first form a simple layer; but they simultaneously consolidate into a definite organ. And the gradual concentration seen in these and other cases is a part of the developmental process—a part which, though more or less recognized by Milne-Edwards and others, does not seem to have been included as an essential element in it.

This progressive integration, manifest alike when tracing up the several stages passed through by every embryo, and when ascending from the lower organic forms to the higher, may be most conveniently studied under several heads. Let us consider first what may be called *longitudinal integration.*

The lower *Annulosa*—worms, myriapods, &c.—are characterized by the great numbers of segments of which they respectively consist, reaching in some cases to several hundreds; but as we advance to the higher *Annulosa*—centipedes, crustaceans, insects, spiders,—we find these numbers greatly reduced, down to twenty-two, thirteen, and even fewer; and accompanying this there is a shortening or integration of the whole body, reaching its extreme in crabs and spiders. Similarly with the development of an individual crustacean or insect. The thorax of a lobster, which, in the adult, forms, with the head, one compact box containing the viscera, is made up by the union of a number of segments which in the embryo were separable. The thirteen distinct divisions seen in the body of a caterpillar, become further integrated in the butterfly: several segments are consolidated to form the thorax, and the abdominal segments are more aggregated than they originally were. The like truth is seen when we pass to the internal organs. In the lower annulose forms, and in the larvæ of the higher ones, the alimentary canal consists either of a tube that is uniform from end to end, or else bulges into a succession of stomachs, one to each segment; but in the developed forms there is a single well-defined stomach. In the nervous, vascular, and respiratory systems a parallel concentration may be traced. Again, in the development of the *Vertebrata* we have sundry examples of longitudinal integration. The coalescence of several segmental groups of bones to form the skull is one instance of it. It is further illustrated in the *os coccygis*, which results from the fusion of a number of caudal vertebræ. And in the consolidation of the sacral vertebræ of a bird it is also well exemplified.

That which we may distinguish as *transverse integration*, is well illustrated among the *Annulosa* in the development of the nervous system. Leaving out those simple forms which do not present distinct ganglia, it is to be observed that the lower annulose animals, in common with the larvæ of the higher, are severally characterized by a double chain of ganglia running from end to end of the body; while in the more advanced annulose animals this double chain becomes a single chain. Mr. Newport has described the course of this concentration in insects; and by Rathke it has been traced in crustaceans. In the early stages of the *Astacus fluviatilis*, or common cray-fish, there is a pair of separate ganglia to each ring. Of the fourteen pairs belonging to the head and thorax, the three pairs in advance of the mouth consolidate into one mass to form the brain, or cephalic ganglion. Meanwhile out of the remainder, the first six pairs severally unite in the median line, while the rest remain more or less separate. Of these six double ganglia thus formed, the anterior four coalesce into one mass; the remaining two coalesce into another mass; and then these two masses coalesce into one. Here we see longitudinal and transverse integration going on simultaneously; and in the highest crustaceans they are both carried still further. The *Vertebrata* exhibit this transverse integration in the development of the generative system. The lowest of the mammalia—the *Monotremata*—in common with birds, have oviducts which towards their lower extremities are dilated into cavities severally performing in an imperfect way the function of a uterus. "In the *Marsupialia*, there is a closer approximation of the two lateral sets of organs on the median line; for the oviducts converge towards one another and meet (without coalescing) on the median line; so that their uterine dilatations are in contact with each other, forming a true 'double uterus.' . . . . As we ascend the series of 'placental' mammals, we find the lateral coalescence becoming gradually more and more complete. . . . . In many of the *Rodentia*, the uterus still remains completely divided into two lateral halves; whilst in others, these coalesce at their lower portion, forming a rudiment of the true 'body' of the uterus in the Human subject. This part increases at the expense of the lateral 'cornua' in the higher Herbivora and Carnivora; but even in the lower Quadrumana, the uterus is somewhat cleft at its summit."* And this process of transverse integration, which is still more striking when observed in its details, is accompanied by parallel though less important changes in the opposite sex. Once more; in the increasing commissural connexion of the cerebral hemispheres, which, though separate in the lower vertebrata, become gradually more united in the higher, we have another instance. And further ones of a different order, but of like general implication, are supplied by the vascular system.

Now it seems to us that the various kinds of integration here exemplified, which are commonly set down as so many independent phenomena, ought to be generalized, and included in the formula describing the process of development. The fact that in an adult crab, many pairs of ganglia originally separate have become fused into a single mass, is a fact only second in significance to the differentiation of its alimentary canal into stomach and intestine. That in the higher *Annulosa*, a single heart replaces the string of rudimentary hearts constituting the dorsal blood-vessel in the lower *Annulosa*, (reaching in one species to the number of one hundred and sixty), is a truth as much needing to be comprised in the history of evolution, as is the formation of a respiratory surface by a branched expansion of the skin. A right conception of the genesis of a vertebral column, includes not only the differentiations from which result the *chorda dorsalis* and the vertebral segments imbedded in it; but quite as much it includes the coalescence of numerous vertebral processes with their respective vertebral bodies. The changes in virtue of which several things become one, demand recognition equally with those in virtue of which one thing becomes several. Evidently, then, the current statement which ascribes the developmental progress to differentiations alone, is incomplete. Adequately to express the facts, we must say that the transition from the homogeneous to the heterogeneous is carried on by differentiations and accompanying integrations.

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* Carpenter's *Principles of Comparative Physiology*, pp. 616-17.

It may not be amiss here to ask—What is the meaning of these integrations? The evidence seems to show that they are in some way dependent on community of function. The eight segments which coalesce to make the head of a centipede, jointly protect the cephalic ganglion, and afford a solid fulcrum for the jaws, &c. The many bones which unite to form a vertebral skull have like uses. In the consolidation of the several pieces which constitute a mammalian pelvis, and in the anchylosis of from ten to nineteen vertebræ in the sacrum of a bird, we have kindred instances of the integration of parts which transfer the weight of the body to the legs. The more or less extensive fusion of the tibia with the fibula and the radius with the ulna in the ungulated mammals, whose habits require only partial rotations of the limbs, is a fact of like meaning. And all the instances lately given—the concentration of ganglia, the replacement of many pulsating blood-sacs by fewer and finally by one, the fusion of two uteri into a single uterus—have the same implication. Whether, as in some cases, the integration is merely a consequence of the growth which eventually brings into contact adjacent parts performing similar duties; or whether, as in other cases, there is an actual approximation of these parts before their union; or whether, as in yet other cases, the integration is of that indirect kind which arises when, out of a number of like organs, one, or a group, discharges an ever-increasing share of the common function, and so grows while the rest dwindle and disappear;—the general fact remains the same, that there is a tendency to the unification of parts having similar duties.

The tendency, however, acts under limiting conditions; and recognition of them will explain some apparent exceptions. In the human fœtus, as in the lower vertebrata, the eyes are placed one on each side of the head. During evolution they become relatively nearer, and at birth are in front; though they are still, in the European infant as in the adult Mongol, proportionately further apart than they afterwards become. But this approximation shows no signs of further increase. Two reasons suggest themselves. One is that the two eyes have not quite the same function, since they are directed to slightly-different aspects of each object looked at; and, since the resulting binocular vision has an advantage over monocular vision, there results a check upon further approach towards identity of function and unity of structure. The other reason is that the interposed structures do not admit of any nearer approach. For the orbits of the eyes to be brought closer together, would imply a decrease in the olfactory chambers; and as these are probably not larger than is demanded by their present functional activity, no decrease can take place. Again, if we trace up the external organs of smell through fishes,* reptiles, ungulate mammals and unguiculate mammals, to man, we perceive a general tendency to coalescence in the median line; and on comparing the savage with the civilized, or the infant with the adult, we see this approach of the nostrils carried furthest in the most perfect of the species. But since the septum which divides them has the function both of an evaporating surface for the lachrymal secretion, and of a ramifying surface for a nerve ancillary to that of smell, it does not disappear entirely: the integration remains incomplete. These and other like instances do not however militate against the hypothesis. They merely show that the tendency is sometimes antagonized by other tendencies. Bearing in mind which qualification, we may say, that as differentiation of parts is connected with difference of function, so there appears to be a connexion between integration of parts and sameness of function.

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* With the exception, perhaps, of the Myxinoid fishes, in which what is considered as the nasal orifice is single, and on the median line. But seeing how unusual is the position of this orifice, it seems questionable whether it is the true homologue of the nostrils.

Closely related to the general truth that the evolution of all organisms is carried on by combined differentiations and integrations, is another general truth, which physiologists appear not to have recognized. When we look at the organic world as a whole, we may observe that, on passing from lower to higher forms, we pass to forms which are not only characterized by a greater differentiation of parts, but are at the same time more completely differentiated from the surrounding medium. This truth may be contemplated under various aspects.

In the first place it is illustrated in *structure.* The advance from the homogeneous to the heterogeneous itself involves an increasing distinction from the inorganic world. In the lowest *Protozoa,* as some of the Rhizopods, we have a homogeneity approaching to that of air, water, or earth; and the ascent to organisms of greater and greater complexity of structure, is an ascent to organisms which are in that respect more strongly contrasted with the relatively structureless masses in the environment.

In *form* again we see the same truth. A general characteristic of inorganic matter is its indefiniteness of form, and this is also a characteristic of the lower organisms, as compared with the higher. Speaking generally, plants are less definite than animals, both in shape and size—admit of greater modifications from variations of position and nutrition. Among animals, the *Amœba* and its allies are not only almost structureless, but are amorphous; and the irregular form is constantly changing. Of the organisms resulting from the aggregation of amœba-like creatures, we find that while some assume a certain definiteness of form, in their compound shells at least, others, as the Sponges, are irregular. In the Zoophytes and in the *Polyzoa,* we see compound organisms, most of which have modes of growth not more determinate than those of plants. But among the higher animals, we find not only that the mature shape of each species is quite definite, but that the individuals of each species differ very little in size.

A parallel increase of contrast is seen in *chemical composition*. With but few exceptions, and those only partial ones, the lowest animal and vegetal forms are inhabitants of the water; and water is almost their sole constituent. Dessicated *Protophyta* and *Protozoa* shrink into mere dust; and among the acalephes we find but a few grains of solid matter to a pound of water. The higher aquatic plants, in common with the higher aquatic animals, possessing as they do much greater tenacity of substance, also contain a greater proportion of the organic elements; and so are chemically more unlike their medium. And when we pass to the superior classes of organisms—land plants and land animals—we find that, chemically considered, they have little in common either with the earth on which they stand or the air which surrounds them.

In *specific gravity*, too, we may note the like. The very simplest forms, in common with the spores and gemmules of the higher ones, are as nearly as may be of the same specific gravity as the water in which they float; and though it cannot be said that among aquatic creatures superior specific gravity is a standard of general superiority, yet we may fairly say that the superior orders of them, when divested of the appliances by which their specific gravity is regulated, differ more from water in their relative weights than do the lower. In terrestrial organisms, the contrast becomes extremely marked. Trees and plants, in common with insects, reptiles, mammals, birds, are all of a specific gravity considerably less than the earth and immensely greater than the air.

We see the law similarly fulfilled in respect of *temperature*.

Plants generate but an extremely small quantity of heat, which is to be detected only by delicate experiments; and practically they may be considered as being in this respect like their environment. Aquatic animals rise very little above the surrounding water in temperature: that of the invertebrata being mostly less than a degree above it, and that of fishes not exceeding it by more than two or three degrees, save in the case of some large red-blooded fishes, as the tunny, which exceed it by nearly ten degrees. Among insects, the range is from two to ten degrees above that of the air: the excess varying according to their activity. The heat of reptiles is from four to fifteen degrees more than that of their medium. While mammals and birds maintain a heat which continues almost unaffected by external variations, and is often greater than that of the air by seventy, eighty, ninety, and even a hundred degrees.

Once more, in greater *self-mobility* a progressive differentiation is traceable. Dead matter is inert: some form of independent motion is our most general test of life. Passing over the indefinite border-land between the animal and vegetable kingdoms, we may roughly class plants as organisms which, while they exhibit the kind of motion implied in growth, are not only without locomotive power, but in nearly all cases are without the power of moving their parts in relation to one another; and thus are less differentiated from the inorganic world than animals. Though in those microscopic *Protophyta* and *Protozoa* inhabiting the water—the spores of algæ, the gemmules of sponges, and the infusoria generally—we see locomotion produced by ciliary action; yet this locomotion, while rapid relatively to their sizes, is absolutely slow. Of the *Cœlenterata*, a great part are either permanently rooted or habitually stationary, and so have scarcely any self-mobility but that implied in the relative movements of parts; while the rest, of which the common jelly-fish serves as a sample, have mostly but little ability to move themselves through the water. Among the higher aquatic *Invertebrata*,—cuttle-fishes and lobsters, for instance,—there is a very considerable power of locomotion; and the aquatic *Vertebrata* are, considered as a class, much more active in their movements than the other inhabitants of the water. But it is only when we come to air-breathing creatures that we find the vital characteristic of self-mobility manifested in the highest degree. Flying insects, mammals, birds, travel with velocities far exceeding those attained by any of the lower classes of animals; and so are more strongly contrasted with their inert environments.

Thus, on contemplating the various grades of organisms in their ascending order, we find them more and more distinguished from their inanimate media in *structure*, in *form*, in *chemical composition*, in *specific gravity*, in *temperature*, in *self-mobility*. It is true that this generalization does not hold with regularity. Organisms which are in some respects the most strongly contrasted with the inorganic world, are in other respects less contrasted than inferior organisms. As a class, mammals are higher than birds; and yet they are of lower temperature, and have smaller powers of locomotion. The stationary oyster is of higher organization than the free-swimming medusa; and the cold-blooded and less heterogeneous fish is quicker in its movements than the warm-blooded and more heterogeneous sloth. But the admission that the several aspects under which this increasing contrast shows itself bear variable ratios to one another, does not negative the general truth enunciated. Looking at the facts in the mass, it cannot be denied that the successively higher groups of organisms are severally characterized, not only by greater differentiation of parts, but also by greater differentiation from the surrounding medium in sundry other physical attributes. It would seem that this peculiarity has some necessary connexion with superior vital manifestations. One of those lowly gelatinous forms which are some of them so transparent and colourless as to be with difficulty distinguished from the water they float in, is not more like its medium in chemical, mechanical, optical, thermal, and other properties, than it is in the passivity with which it submits to all the actions brought to bear on it; while the mammal does not more widely differ from inanimate things in these properties than it does in the activity with which it meets surrounding changes by compensating changes in itself. Between these two extremes, we see a tolerably constant ratio between these two kinds of contrast. In proportion as an organism is physically like its environment it remains a passive partaker of the changes going on in its environment; while in proportion as it is endowed with powers of counteracting such changes, it exhibits greater unlikeness to its environment.

Thus far we have proceeded inductively, in conformity with established usage; but it seems to us that much may be done in this and other departments of biologic inquiry by pursuing the deductive method. The generalizations at present constituting the science of physiology, both general and special, have been reached *a posteriori;* but certain fundamental data have now been discovered, starting from which we may reason our way *a priori,* not only to some of the truths that have been ascertained by observation and experiment, but also to some others. The possibility of such *a priori* conclusions will be at once recognized on considering some familiar cases.

Chemists have shown that a necessary condition to vital activity in animals is oxidation of certain matters contained in the body either as components or as waste products. The oxygen requisite for this oxidation is contained in the surrounding medium—air or water, as the case may be. If the organism be minute, mere contact of its external surface with the oxygenated medium achieves the requisite oxidation; but if the organism is bulky, and so exposes a surface which is small in proportion to its mass, any considerable oxidation cannot be thus achieved. One of two things is therefore implied. Either this bulky organism, receiving no oxygen but that absorbed through its integument, must possess but little vital activity; or else, if it possesses much vital activity, there must be some extensive ramified surface, internal or external, through which adequate aeration may take place—a respiratory apparatus. That is to say, lungs, or gills, or branchiæ, or their equivalents, are predicable *a priori* as possessed by all active creatures of any size.

Similarly with respect to nutriment. There are *entozoa* which, living in the insides of other animals, and being constantly bathed by nutritive fluids, absorb a sufficiency through their outer surfaces; and so have no need of stomachs, and do not possess them. But all other animals, inhabiting media that are not in themselves nutritive, but only contain masses of food here and there, must have appliances by which these masses of food may be utilized. Evidently mere external contact of a solid organism with a solid portion of nutriment, could not result in the absorption of it in any moderate time, if at all. To effect absorption, there must be both a solvent or macerating action, and an extended surface fit for containing and imbibing the dissolved products: there must be a digestive cavity. Thus, given the ordinary conditions of animal life, and the possession of stomachs by all creatures living under these conditions may be deductively known.

Carrying out the train of reasoning still further, we may infer the existence of a vascular system or something equivalent to it, in all creatures of any size and activity. In a comparatively small inert animal, such as the hydra, which consists of little more than a sac having a double wall—an outer layer of cells forming the skin, and an inner layer forming the digestive and absorbent surface—there is no need for a special apparatus to diffuse through the body the aliment taken up; for the body is little more than a wrapper to the food it encloses. But where the bulk is considerable, or where the activity is such as to involve much waste and repair, or where both these characteristics exist, there is a necessity for a system of blood-vessels. It is not enough that there be adequately extensive surfaces for absorption and aeration; for in the absence of any means of conveyance, the absorbed elements can be of little or no use to the organism at large. Evidently there must be channels of communication. When, as in the *Medusæ*, we find these channels of communication consisting simply of branched canals opening out of the stomach and spreading through the disk, we may know, *a priori*, that such creatures are comparatively inactive; seeing that the nutritive liquid thus partially distributed throughout their bodies is crude and dilute, and that there is no efficient appliance for keeping it in motion. Conversely, when we meet with a creature of considerable size which displays much vivacity, we may know, *a priori*, that it must have an apparatus for the unceasing supply of concentrated nutriment, and of oxygen, to every organ—a pulsating vascular system.

It is manifest, then, that setting out from certain known fundamental conditions to vital activity, we may deduce from them sundry of the chief characteristics of organized bodies. Doubtless these known fundamental conditions have been inductively established. But what we wish to show is that, given these inductively-established primary facts in physiology, we may with safety draw certain general deductions from them. And, indeed, the legitimacy of such deductions, though not formally acknowledged, is practically recognized in the convictions of every physiologist, as may be readily proved. Thus, were a physiologist to find a creature exhibiting complex and variously co-ordinated movements, and yet having no nervous system; he would be less astonished at the breach of his empirical generalization that all such creatures have nervous systems, than at the disproof of his unconscious deduction that all creatures exhibiting complex and variously co-ordinated movements must have an "internuncial" apparatus by which the co-ordination may be effected. Or were he to find a creature having blood rapidly circulated and rapidly aerated, but yet showing a low temperature, the proof so afforded that active change of matter is not, as he had inferred from chemical data, the cause of animal heat, would stagger him more than would the exception to a constantly-observed relation. Clearly, then, the *a priori* method already plays a part in physiological reasoning. If not ostensibly employed as a means of reaching new truths, it is at least privately appealed to for confirmation of truths reached *a posteriori*.

But the illustrations above given go far to show, that it may to a considerable extent be safely used as an independent instrument of research. The necessities for a nutritive system, a respiratory system, and a vascular system, in all animals of size and vivacity, seem to us legitimately inferable from the conditions to continued vital activity. Given the physical and chemical data, and these structural peculiarities may be deduced with as much certainty as may the hollowness of an iron ball from its power of floating in water.

It is not, of course, asserted that the more *special* physiological truths can be deductively reached. The argument by no means implies this. Legitimate deduction presupposes adequate data; and in respect to the *special* phenomena of organic growth, structure, and function, adequate data are unattainable, and will probably ever remain so. It is only in the case of the more *general* physiological truths, such as those above instanced, where we have something like adequate data, that deductive reasoning becomes possible.

And here is reached the stage to which the foregoing considerations are introductory. We propose now to show that there are certain still more general attributes of organized bodies, which are deducible from certain still more general attributes of things.

In an essay on "Progress: its Law and Cause," elsewhere published,* we have endeavoured to show that the transformation of the homogeneous into the heterogeneous, in which all progress, organic or other, essentially consists, is consequent on the production of many effects by one cause—many changes by one force. Having pointed out that this is a law of all things, we proceeded to show deductively that the multiform evolutions of the homogeneous into the heterogeneous—astronomic, geologic, ethnologic, social, &c.,—were explicable as consequences. And though in the case of organic evolution, lack of data disabled us from specifically tracing out the progressive complication as due to the multiplication of effects; yet, we found sundry indirect evidences that it was so. Now in so far as this conclusion, that organic evolution results from the decomposition of each expended force into several forces, was inferred from the general law previously pointed out, it was an example of deductive physiology. The particular was concluded from the universal.

We here propose in the first place to show, that there is another general truth closely connected with the above; and in common with it underlying explanations of all progress, and therefore the progress of organisms—a truth which may indeed be considered as taking precedence of it in respect of time, if not in respect of generality. This truth is, that *the condition of homogeneity is a condition of unstable equilibrium.*

The phrase *unstable equilibrium* is one used in mechanics to express a balance of forces of such kind, that the interference of any further force, however minute, will destroy the arrangement previously existing, and bring about a different arrangement. Thus, a stick poised on its lower end is in unstable equilibrium: however exactly it may be placed in a perpendicular position, as soon as it is left to itself it begins, at first imperceptibly and then visibly, to lean on one side, and with increasing rapidity falls into another position. Conversely, a stick suspended from its upper end is in stable equilibrium: however much disturbed, it will return to the same position. Our meaning is, then, that the state of homogeneity, like the state of the stick poised on its lower end, is one that cannot be maintained; and that hence results the first step in its gravitation towards the heterogeneous. Let us take a few illustrations.

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* In the *Westminster Review* for April, 1857; and now reprinted in this volume.

Of mechanical ones the most familiar is that of the scales. If accurately made and not clogged by dirt or rust, a pair of scales cannot be perfectly balanced: eventually one scale will descend and the other ascend—they will assume a heterogeneous relation. Again, if we sprinkle over the surface of a liquid a number of equal-sized particles, having an attraction for one another, they will, no matter how uniformly distributed, by and by concentrate irregularly into groups. Were it possible to bring a mass of water into a state of perfect homogeneity—a state of complete quiescence, and exactly equal density throughout—yet the radiation of heat from neighbouring bodies, by affecting differently its different parts, would soon produce inequalities of density and consequent currents; and would so render it to that extent heterogeneous. Take a piece of red-hot matter, and however evenly heated it may at first be, it will quickly cease to be so: the exterior, cooling faster than the interior, will become different in temperature from it. And the lapse into heterogeneity of temperature, so obvious in this extreme case, is ever taking place more or less in all cases. The actions of chemical forces supply other illustrations. Expose a fragment of metal to air or water, and in course of time it will be coated with a film of oxide, carbonate, or other compound: its outer parts will become unlike its inner parts. Thus, every homogeneous aggregate of matter tends to lose its balance in some way or other—either mechanically, chemically, thermally or electrically; and the rapidity with which it lapses into a non-homogeneous state is simply a question of time and circumstances. Social bodies illustrate the law with like constancy. Endow the members of a community with equal properties, positions, powers, and they will forthwith begin to slide into inequalities. Be it in a representative assembly, a railway board, or a private partnership, the homogeneity, though it may continue in name, inevitably disappears in reality.

The instability thus variously illustrated becomes still more manifest if we consider its rationale. It is consequent on the fact that the several parts of any homogeneous mass are necessarily exposed to different forces—forces which differ either in their kinds or amounts; and being exposed to different forces they are of necessity differently modified. The relations of outside and inside, and of comparative nearness to neighbouring sources of influence, imply the reception of influences which are unlike in quantity or quality or both; and it follows that unlike changes will be wrought in the parts dissimilarly acted upon. The unstable equilibrium of any homogeneous aggregate can thus be shown both inductively and deductively.

And now let us consider the bearing of this general truth on the evolution of organisms. The germ of a plant or animal is one of these homogeneous aggregates—relatively homogeneous if not absolutely so—whose equilibrium is unstable. But it has not simply the ordinary instability of homogeneous aggregates: it has something more. For it consists of units which are themselves specially characterized by instability. The constituent molecules of organic matter are distinguished by the feebleness of the affinities which hold their component elements together. They are extremely sensitive to heat, light, electricity, and the chemical actions of foreign elements; that is, they are peculiarly liable to be modified by disturbing forces. Hence then it follows, *a priori*, that a homogeneous aggregate of these unstable molecules will have an excessive tendency to lose its equilibrium. It will have a quite special liability to lapse into a non-homogeneous state. It will rapidly gravitate towards heretogeneity.

Moreover, the process must repeat itself in each of the subordinate groups of organic units which are differentiated by the modifying forces. Each of these subordinate groups, like the original group, must gradually, in obedience to the influences acting on it, lose its balance of parts—must pass from a uniform into a multiform state. And so on continuously.

Thus, starting from the general laws of things, and the known chemical attributes of organic matter, we may conclude deductively that the homogeneous germs of organisms have a peculiar proclivity towards a non-homogeneous state; which may be either the state we call decomposition, or the state we call organization.

At present we have reached a conclusion only of the most general nature. We merely learn that *some* kind of heterogeneity is inevitable; but as yet there is nothing to tell us *what* kind. Besides that *orderly* heterogeneity which distinguishes organisms, there is the *disorderly* or *chaotic* heterogeneity, into which a loose mass of inorganic matter lapses; and at present no reason has been given why the homogeneous germ of a plant or animal should not lapse into the disorderly instead of the orderly heterogeneity. But by pursuing still further the line of argument hitherto followed we shall find a reason.

We have seen that the instability of homogeneous aggregates in general, and of organic ones in particular, is consequent on the various ways and degrees in which their constituent parts are exposed to the disturbing forces brought to bear on them: their parts are differently acted upon, and therefore become different. Manifestly, then, a rationale of the special changes which a germ undergoes, must be sought in the particular relations which its several parts bear to each other and to their environment. However it may be masked, we may suspect the fundamental principle of organization to be, that the many like units forming a germ acquire those kinds and degrees of unlikeness which their respective positions entail.

Take a mass of unorganized but organizable matter—either the body of one of the lowest living forms, or the germ of one of the higher. Consider its circumstances. It is immersed in water or air; or it is contained within a parent organism. Wherever placed, however, its outer and inner parts stand differently related to surrounding existences—nutriment, oxygen, and the various stimuli. But this is not all. Whether it lies quiescent at the bottom of the water, whether it moves through the water preserving some definite attitude, or whether it is in the inside of an adult; it equally results that certain parts of its surface are more directly exposed to surrounding agencies than other parts—in some cases more exposed to light, heat, or oxygen, and in others to the maternal tissues and their contents. The destruction of its original equilibrium is therefore certain. It may take place in one of two ways. Either the disturbing forces may be such as to overbalance the affinities of the organic elements, in which case there results that chaotic heterogeneity known as decomposition; or, as is ordinarily the case, such changes are induced as do not destroy the organic compounds, but only modify them: the parts most exposed to the modifying forces being most modified. Hence result those first differentiations which constitute incipient organization. From the point of view thus reached, suppose we look at a few cases: neglecting for the present all consideration of the tendency to assume the inherited type.

Note first what appear to be exceptions, as the *Amœba*. In this creature and its allies, the substance of the jelly-like body remains throughout life unorganized—undergoes no permanent differentiations. But this fact, which seems directly opposed to our inference, is really one of the most significant evidences of its truth. For what is the peculiarity of the Rhizopods, exemplified by the *Amœba?* They undergo perpetual and irregular changes of shape—they show no persistent relations of parts. What lately formed a portion of the interior is now protruded, and, as a temporary limb, is attached to some object it happens to touch. What is now a part of the surface will presently be drawn, along with the atom of nutriment sticking to it, into the centre of the mass. Thus there is an unceasing interchange of places; and the relations of inner and outer have no settled existence. But by the hypothesis, it is only in virtue of their unlike positions with respect to modifying forces, that the originally-like units of a living mass become unlike. We must not therefore expect any established differentiation of parts in creatures which exhibit no established differences of position in their parts.

This negative evidence is borne out by abundant positive evidence. When we turn from these ever-changing specks of living jelly to organisms having unchanging distributions of substance, we find differences of tissue corresponding to differences of relative position. In all the higher *Protozoa*, as also in the *Protophyta*, we meet with a fundamental differentiation into cell-membrane and cell-contents, answering to that fundamental contrast of conditions implied by the words outside and inside. And on passing from what are roughly classed as unicellular organisms to the lowest of those which consist of aggregated cells, we equally observe the connexion between structural differences and differences of circumstance. In the sponge, permeated throughout by currents of sea-water, the absence of definite organization corresponds with the absence of definite unlikeness of conditions. In the *Thalassicolla* of Professor Huxley—a transparent, colourless body, found floating passively at the surface of the sea, and consisting essentially of "a mass of cells united by jelly"—there is displayed a rude structure obviously subordinated to the primary relations of centre and surface: in all of its many and important varieties, the parts exhibit a more or less concentric arrangement.

After this primary modification, by which the outer tissues are differentiated from the inner, the next in order of constancy and importance is that by which some part of the outer tissues is differentiated from the rest; and this corresponds with the almost universal fact that some part of the outer tissues is more directly exposed to certain environing influences than the rest. Here, as before, the apparent exceptions are extremely significant. Some of the lowest vegetable organisms, as the *Hematococci* and *Protococci*, evenly imbedded in a mass of mucus, or dispersed through the Arctic snow, display no differentiations of surface: the several parts of the surface being subjected to no definite contrasts of conditions. The *Thalassicolla* above mentioned, unfixed, and rolled about by the waves, presents all its sides successively to the same agencies; and all its sides are alike. A ciliated sphere like the *Volvox* has no parts of its periphery unlike other parts; and it is not to be expected that it should have; seeing that as it revolves in all directions, it does not, in traversing the water, permanently expose any part to special conditions. But when we come to creatures that are either fixed, or while moving, severally preserve a definite attitude, we no longer find uniformity of surface. The gemmule of a Zoophyte, which during its locomotive stage is distinguishable only into outer and inner tissues, no sooner takes root than its upper end begins to assume a different structure from its lower. The free-swimming embryo of an aquatic annelid, being ovate and not ciliated all over, moves with one end foremost; and its differentiations proceed in conformity with this contrast of circumstances.

The principle thus displayed in the humbler forms of life, is traceable during the development of the higher; though being here soon masked by the assumption of the hereditary type, it cannot be traced far. Thus the "mulberry-mass" into which a fertilized ovum of a vertebrate animal first resolves itself, soon begins to exhibit a difference between the outer and inner parts answering to the difference of circumstances. The peripheral cells, after reaching a more complete development than the central ones, coalesce into a membrane enclosing the rest; and then the cells lying next to these outer ones become aggregated with them, and increase the thickness of the germinal membrane, while the central cells liquefy. Again, one part of the germinal membrane presently becomes distinguishable as the germinal spot; and without asserting that the cause of this is to be found in the unlike relations which the respective parts of the germinal membrane bear to environing influences, it is clear that we have in these unlike relations an element of disturbance tending to destroy the original homogeneity of the germinal membrane. Further, the germinal membrane by and by divides into two layers, internal and external; the one in contact with the liquefied interior part or yelk, the other exposed to the surrounding fluids: this contrast of circumstances being in obvious correspondence with the contrast of structures which follows it. Once more, the subsequent appearance of the vascular layer between these mucous and serous layers, as they have been named, admits of a like interpretation. And in this and the various complications which now begin to show themselves, we may see coming into play that general law of the multiplication of effects flowing from one cause, to which the increase of heterogeneity was elsewhere ascribed.*

Confining our remarks, as we do, to the most general facts of development, we think that some light is thus thrown on them. That the unstable equilibrium of a homogeneous germ must be destroyed by the unlike exposure of its several units to surrounding influences, is an *a priori* conclusion. And it seems also to be an *a priori* conclusion, that the several units thus differently acted upon, must either be decomposed, or must undergo such modifications of nature as may enable them to live in the respective circumstances they are thrown into: in other words—*they must either die or become adapted to their conditions.* Indeed, we might infer as much without going through the foregoing train of reasoning. The superficial organic units (be they the outer cells of a "mulberry-mass," or be they the outer molecules of an individual cell) must assume the function which their position necessitates; and assuming this function, must acquire such character as performance of it involves. The layer of organic units lying in contact with the yelk must be those through which the yelk is absorbed; and so must be adapted to the absorbent office. On this condition only does the process of organization appear possible. We might almost say that just as some race of animals, which multiplies and spreads into divers regions of the earth, becomes differentiated into several races through the adaptation of each to its conditions of life; so, the originally homogeneous population of cells arising in a fertilized germ-cell, becomes divided into several populations of cells that grow unlike in virtue of the unlikeness of their circumstances.

Moreover, it is to be remarked in further proof of our position, that it finds its clearest and most abundant illustrations where the conditions of the case are the simplest and most general—where the phenomena are the least involved: we mean in the production of individual cells. The structures which presently arise round nuclei in a blastema, and which have in some way been determined by those nuclei as centres of influence, evidently conform to the law; for the parts of the blastema in contact with the nuclei are differently conditioned from the parts not in contact with them. Again, the formation of a membrane round each of the masses of granules into which the endochrome of an alga-cell breaks up, is an instance of analogous kind. And should the recently-asserted fact that cells may arise round vacuoles in a mass of organizable substance, be confirmed, another good example will be furnished; for such portions of substance as bound these vacant spaces are subject to influences unlike those to which other portions of the substance are subject. If then we can most clearly trace this law of modification in these primordial processes, as well as in those more complex but analogous ones exhibited in the early changes of an ovum, we have strong reason for thinking that the law is fundamental.

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* See Essay on "Progress: its Law and Cause."

But, as already more than once hinted, this principle, understood in the simple form here presented, supplies no key to the detailed phenomena of organic development. It fails entirely to explain generic and specific peculiarities; and leaves us equally in the dark respecting those more important distinctions by which families and orders are marked out. Why two ova, similarly exposed in the same pool, should become the one a fish, and the other a reptile, it cannot tell us. That from two different eggs placed under the same hen, should respectively come forth a duckling and a chicken, is a fact not to be accounted for on the hypothesis above developed. Here we are obliged to fall back upon the unexplained principle of hereditary transmission. The capacity possessed by an unorganized germ of unfolding into a complex adult which repeats ancestral traits in minute details, and that even when it has been placed in conditions unlike those of its ancestors, is a capacity impossible for us to understand. That a microscopic portion of seemingly structureless matter should embody an influence of such kind, that the resulting man will in fifty years after become gouty or insane, is a truth which would be incredible were it not daily illustrated. But though the *manner* in which hereditary likeness, in all its complications, is conveyed, is a mystery passing comprehension, it is quite conceivable that it is conveyed in subordination to the law of adaptation above explained; and we are not without reasons for thinking that it is so. Various facts show that acquired peculiarities resulting from the adaptation of constitution to conditions, are transmissible to offspring. Such acquired peculiarities consist of differences of structure or composition in one or more of the tissues. That is to say, of the aggregate of similar organic units composing a germ, the group going to the formation of a particular tissue, will take on the special character which the adaptation of that tissue to new circumstances had produced in the parents. We know this to be a general law of organic modifications. Further, it is the *only* law of organic modifications of which we have any evidence.* It is not impossible then that it is the universal law; comprehending not simply those minor modifications which offspring inherit from recent ancestry, but comprehending also those larger modifications distinctive of species, genus, order, class, which they inherit from antecedent races of organisms. And thus it *may be* that the law of adaptation is the sole law; presiding not only over the differentiation of any race of organisms into several races, but also over the differentiation of the race of organic units composing a germ, into the many races of organic units composing an adult. So understood, the process gone through by every unfolding organism will consist, partly in the direct adaptation of its elements to their several circumstances, and partly in the assumption of characters resulting from analogous adaptations of the elements of all ancestral organisms.

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* This was written before the publication of the *Origin of Species*. I leave it standing because it shows the stage of thought then arrived at.

But our argument does not commit us to any such far-reaching speculation as this; which we introduce simply as suggested by it, not involved. All we are here concerned to show, is, that the deductive method aids us in interpreting some of the more general phenomena of development. That all homogeneous aggregates are in unstable equilibrium is a universal truth, from which is deducible the instability of every organic germ. From the known sensitiveness of organic compounds to chemical, thermal, and other disturbing forces, we further infer the *unusual* instability of every organic germ—a proneness far beyond that of other homogeneous aggregates to lapse into a heterogeneous state. By the same line of reasoning we are led to the additional inference, that the first divisions into which a germ resolves itself, being severally in a state of unstable equilibrium, are similarly prone to undergo further changes; and so on continuously. Moreover, we have found it to be equally an *a priori* conclusion, that as, in all other cases, the loss of homogeneity is due to the different degrees and kinds of force brought to bear on the different parts; so, in this case too, difference of circumstances is the primary cause of differentiation. Add to which, that as the several changes undergone by the respective parts thus diversely acted upon, are changes which do not destroy their vital activity, they must be changes which bring that vital activity into subordination to the incident forces—they must be adaptations; and the like must be in some sense true of all the subsequent changes. Thus by deductive reasoning we get some insight into the method of organization. However unable we are, and probably ever shall be, to comprehend the way in which a germ is made to take on the special form of its race, we may yet comprehend the general principles which regulate its first modifications; and, remembering the unity of plan so conspicuous throughout nature, we may *suspect* that these principles are in some way concerned in succeeding modifications.

A controversy now going on among zoologists, opens yet another field for the application of the deductive method. We believe that the question whether there does or does not exist a *necessary correlation* among the several parts of an organism is determinable *a priori*.

Cuvier, who first asserted this necessary correlation, professed to base his restorations of extinct animals upon it. Geoffroy St. Hilaire and De Blainville, from different points of view, contested Cuvier's hypothesis; and the discussion, which has much interest as bearing on paleontology, has been recently revived under a somewhat modified form: Professors Huxley and Owen being respectively the assailant and defender of the hypothesis.

Cuvier says—"Comparative anatomy possesses a principle whose just development is sufficient to dissipate all difficulties; it is that of the correlation of forms in organized beings, by means of which every kind of organized being might, strictly speaking, be recognized by a fragment of any of its parts. Every organized being constitutes a whole, a single and complete system, whose parts mutually correspond and concur by their reciprocal reaction to the same definite end. None of these parts can be changed without affecting the others; and consequently each taken separately, indicates and gives all the rest." He then gives illustrations: arguing that the carnivorous form of tooth necessitating a certain action of the jaw, implies a particular form in its condyles; implies also limbs fit for seizing and holding prey; therefore implies claws, a certain structure of the leg-bones, a certain form of shoulder-blade. Summing up he says, that "the claw, the scapula, the condyle, the femur, and all the other bones, taken separately, will give the tooth or one another; and by commencing with any one, he who had a rational conception of the laws of the organic economy, could reconstruct the whole animal."

It will be seen that the method of restoration here contended for, is based on the alleged physiological necessity of the connexion between these several peculiarities. The argument used is, not that a scapula of a certain shape may be recognized as having belonged to a carnivorous mammal because we always find that carnivorous mammals *do* possess such scapulas; but the argument is that they *must* possess them, because carnivorous habits would be impossible without them. And in the above quotation Cuvier asserts that the necessary correlation which he considers so obvious in these cases, exists throughout the system: admitting, however, that in consequence of our limited knowledge of physiology we are unable in many cases to trace this necessary correlation, and are obliged to base our conclusions upon observedcoexistences, of which we do not understand the reason, but which we find invariable.

Now Professor Huxley has recently shown that, in the first place, this empirical method, which Cuvier introduces as quite subordinate, and to be used only in aid of the rational method, is really the method which Cuvier habitually employed—the so-called rational method remaining practically a dead letter; and, in the second place, he has shown that Cuvier himself has in several places so far admitted the inapplicability of the rational method, as virtually to surrender it as a method. But more than this, Professor Huxley contends that the alleged necessary correlation is not true. Quite admitting the physiological dependence of parts on each other, he denies that it is a dependence of a kind which could not be otherwise. "Thus the teeth of a lion and the stomach of the animal are in such relation that the one is fitted to digest the food which the other can tear, they are physiologically correlated; but we have no reason for affirming this to be a necessary physiological correlation, in the sense that no other could equally fit its possessor for living on recent flesh. The number and form of the teeth might have been quite different from that which we know them to be, and the construction of the stomach might have been greatly altered; and yet the functions of these organs might have been equally well performed."

Thus much is needful to give an idea of the controversy. It is not here our purpose to go more at length into the evidence cited on either side. We simply wish to show that the question may be settled deductively. Before going on to do this, however, let us briefly notice two collateral points.

In his defence of the Cuvierian doctrine, Professor Owen avails himself of the *odium theologicum*. He attributes to his opponents "the insinuation and masked advocacy of the doctrine subversive of a recognition of the Higher Mind." Now, saying nothing about the questionable propriety of thus prejudging an issue in science, we think this is an unfortunate accusation. What is there in the hypothesis of *necessary*, as distinguished from *actual*, correlation of parts, which is particularly in harmony with Theism? Maintenance of the *necessity*, whether of sequences or of coexistences, is commonly thought rather a derogation from divine power than otherwise. Cuvier says—"None of these parts can be changed without affecting the others; and consequently, each taken separately, indicates and gives all the rest." That is to say, in the nature of things the correlation *could not* have been otherwise. On the other hand, Professor Huxley says we have no warrant for asserting that the correlation *could not* have been otherwise; but have not a little reason for thinking that the same physiological ends might have been differently achieved.

The one doctrine limits the possibilities of creation; the other denies the implied limit. Which, then, is most open to the charge of covert Atheism?

On the other point we lean to the opinion of Professor Owen. We agree with him in thinking that where a rational correlation (in the highest sense of the term) can be made out, it affords a better basis for deduction than an empirical correlation ascertained only by accumulated observations. Premising that by rational correlation is not meant one in which we can trace, or think we can trace, a design, but one of which the negation is inconceivable (and this is the species of correlation which Cuvier's principle implies); then we hold that our knowledge of the correlation is of a more certain kind than where it is simply inductive. We think that Professor Huxley, in his anxiety to avoid the error of making Thought the measure of Things, does not sufficiently bear in mind the fact, that as our notion of necessity is determined by some absolute uniformity pervading all orders of our experiences, it follows that an organic correlation which cannot be conceived otherwise, is guaranteed by a much wider induction than one ascertained only by the observation of organisms. But the truth is, that there are relatively few organic correlations of which the negation is inconceivable. If we find the skull, vertebræ, ribs, and phalanges of some quadruped as large as an elephant; we may indeed be certain that the legs of this quadruped were of considerable size—much larger than those of a rat; and our reason for conceiving this correlation as necessary, is, that it is based, not only upon our experiences of moving organisms, but upon all our mechanical experiences relative to masses and their supports. But even were there many physiological correlations really of this order, which there are not, there would be danger in pursuing this line of reasoning, in consequence of the liability to include within the class of truly necessary correlations, those which are not such. For instance, there would seem to be a necessary correlation between the eye and the surface of the body: light being needful for vision, it might be supposed that every eye must be external. Nevertheless it is a fact that there are creatures, as the *Cirrhipœdia*, having eyes (not very efficient ones, it may be) deeply imbedded within the body. Again, a necessary correlation might be assumed between the dimensions of the mammalian uterus and those of the pelvis. It would appear impossible that in any species there should exist a well-developed uterus containing a full-sized fœtus, and yet that the arch of the pelvis should be too small to allow the fœtus to pass. And were the only mammal having a very small pelvic arch, a fossil one, it would have been inferred, on the Cuvierian method, that the fœtus must have been born in a rudimentary state; and that the uterus must have been proportionally small. But there happens to be an extant mammal having an undeveloped pelvis—the mole—which presents us with a fact that saves us from this erroneous inference. The young of the mole are not born through the pelvic arch at all; but in front of it! Thus, granting that some quite *direct* physiological correlations may be necessary, we see that there is great risk of including among them some which are not.

With regard to the great mass of the correlations, however, including all the *indirect* ones, Professor Huxley seems to us warranted in denying that they are necessary; and we now propose to show deductively the truth of his thesis. Let us begin with an analogy.

Whoever has been through an extensive iron-works, has seen a gigantic pair of shears worked by machinery, and used for cutting in two, bars of iron that are from time to time thrust between its blades. Supposing these blades to be the only visible parts of the apparatus, anyone observing their movements (or rather the movement of one, for the other is commonly fixed), will see from the manner in which the angle increases and decreases, and from the curve described by the moving extremity, that there must be some centre of motion—either a pivot or an external box equivalent to it. This may be regarded as a necessary correlation. Moreover, he might infer that beyond the centre of motion the moving blade was produced into a lever, to which the power was applied; but as another arrangement is just possible, this could not be called anything more than a highly probable correlation. If now he went a step further, and asked how the reciprocal movement was given to the lever, he would perhaps conclude that it was given by a crank. But if he knew anything of mechanics, he would know that it might possibly be given by an eccentric. Or again, he would know that the effect could be achieved by a cam. That is to say, he would see that there was no necessary correlation between the shears and the remoter parts of the apparatus. Take another case. The plate of a printing-press is required to move up and down to the extent of an inch or so; and it must exert its greatest pressure when it reaches the extreme of its downward movement. If now anyone will look over the stock of a printing-press maker, he will see half a dozen different mechanical arrangements by which these ends are achieved; and a machinist would tell him that as many more might readily be invented. If, then, there is no necessary correlation between the special parts of a machine, still less is there between those of an organism.

From a converse point of view the same truth is manifest. Bearing in mind the above analogy, it will be foreseen that an alteration in one part of an organism will not necessarily entail *some one specific set of alterations in the other parts.* Cuvier says, "None of these parts can be changed without affecting the others; and consequently, each taken separately, indicates and gives all the rest." The first of these propositions may pass, but the second, which it is alleged follows from it, is not true; for it implies that "all the rest" can be severally affected in only one way and degree, whereas they can be affected in many ways and degrees. To show this, we must again have recourse to a mechanical analogy.

If you set a brick on end and thrust it over, you can predict with certainty in what direction it will fall, and what attitude it will assume. If, again setting it up, you put another on the top of it, you can no longer foresee with accuracy the results of an overthrow; and on repeating the experiment, no matter how much care is taken to place the bricks in the same positions, and to apply the same degree of force in the same direction, the effects will on no two occasions be exactly alike. And in proportion as the aggregation is complicated by the addition of new and unlike parts, will the results of any disturbance become more varied and incalculable. The like truth is curiously illustrated by locomotive engines. It is a fact familiar to mechanical engineers and engine-drivers, that out of a number of engines built as accurately as possible to the same pattern, no two will act in just the same manner. Each will have its peculiarities. The play of actions and reactions will so far differ, that under like conditions each will behave in a somewhat different way; and every driver has to learn the idiosyncrasies of his own engine before he can work it to the greatest advantage. In organisms themselves this indefiniteness of mechanical reaction is clearly traceable. Two boys throwing stones will always differ more or less in their attitudes, as will two billiard-players. The familiar fact that each individual has a characteristic gait, illustrates the point still better. The rhythmical motion of the leg is simple, and on the Cuvierian hypothesis, should react on the body in some uniform way. But in consequence of those slight differences of structure which consist with identity of species, no two individuals make exactly similar movements either of the trunk or the arms. There is always a peculiarity recognizable by their friends.

When we pass to disturbing forces of a non-mechanical kind, the same truth becomes still more conspicuous. Expose several persons to a drenching storm; and while one will subsequently feel no appreciable inconvenience, another will have a cough, another a catarrh, another an attack of diarrhœa, another a fit of rheumatism. Vaccinate several children of the same age with the same quantity of virus, applied to the same part, and the symptoms will not be quite alike in any of them, either in kind or intensity; and in some cases the differences will be extreme. The quantity of alcohol which will send one man to sleep, will render another unusually brilliant—will make this maudlin, and that irritable. Opium will produce either drowsiness or wakefulness: so will tobacco.

Now in all these cases—mechanical and other—some force is brought to bear primarily on one part of an organism, and secondarily on the rest; and, according to the doctrine of Cuvier, the rest ought to be affected in a specific way. We find this to be by no means the case. The original change produced in one part does not stand in any necessary correlation with every one of the changes produced in the other parts; nor do these stand in any necessary correlation with one another. The functional alteration which the disturbing force causes in the organ directly acted upon, does not involve some *particular set* of functional alterations in the other organs; but will be followed by some one out of various sets. And it is a manifest corollary, that any *structural alteration* which may eventually be produced in the one organ, will not be accompanied by *some particular set of structural alterations* in the other organs. There will be no necessary correlation of forms.

Thus Paleontology must depend upon the empirical method. A fossil species that was obliged to change its food or habits of life, did not of necessity undergo the particular set of modifications exhibited; but, under some slight change of predisposing causes—as of season or latitude—might have undergone some other set of modifications: the determining circumstance being one which, in the human sense, we call fortuitous.

May we not say then, that the deductive method elucidates this vexed question in physiology; while at the same time our argument collaterally exhibits the limits within which the deductive method is applicable. For while we see that this extremely *general* question may be satisfactorily dealt with deductively; the conclusion arrived at itself implies that the more *special* phenomena of organization cannot be so dealt with.

There is yet another method of investigating the general truths of physiology—a method to which physiology already owes one luminous idea, but which is not at present formally recognized as a method. We refer to the comparison of physiological phenomena with social phenomena.

The analogy between individual organisms and the social organism, is one that has from early days occasionally forced itself on the attention of the observant. And though modern science does not countenance those crude ideas of this analogy which have been from time to time expressed since the Greeks flourished; yet it tends to show that there *is* an analogy, and a remarkable one. While it is becoming clear that there are not those special parallelisms between the constituent parts of a man and those of a nation, which have been thought to exist; it is also becoming clear that the general principles of development and structure displayed in organized bodies are displayed in societies also. The fundamental characteristic both of societies and of living creatures, is, that they consist of mutually-dependent parts; and it would seem that this involves a community of various other characteristics. Those who are acquainted with the broad facts of both physiology and sociology, are beginning to recognize this correspondence not as a plausible fancy, but as a scientific truth. And we are strongly of opinion that it will by and by be seen to hold to an extent which few at present suspect.

Meanwhile, if any such correspondence exists, it is clear that physiology and sociology will more or less interpret each other. Each affords its special facilities for inquiry. Relations of cause and effect clearly traceable in the social organism, may lead to the search for analogous ones in the individual organism; and may so elucidate what might else be inexplicable. Laws of growth and function disclosed by the pure physiologist, may occasionally give us the clue to certain social modifications otherwise difficult to understand. If they can do no more, the two sciences can at least exchange suggestions and confirmations; and this will be no small aid. The conception of "the physiological division of labour," which political economy has already supplied to physiology, is one of no small value. And probably it has others to give.

In support of this opinion, we will now cite cases in which such aid is furnished. And in the first place, let us see whether the facts of social organization do not afford additional support to some of the doctrines set forth in the foregoing parts of this article.

One of the propositions supported by evidence was that in animals the process of development is carried on, not by differentiations only, but by subordinate integrations. Now in the social organism we may see the same duality of process; and further, it is to be observed that the integrations are of the same three kinds. Thus we have integrations which arise from the simple growth of adjacent parts that perform like functions: as, for instance, the coalescence of Manchester with its calico-weaving suburbs. We have other integrations which arise when, out of several places producing a particular commodity, one monopolizes more and more of the business, and leaves the rest to dwindle: witness the growth of the Yorkshire cloth-districts at the expense of those in the west of England; or the absorption by Staffordshire of the pottery-manufacture, and the consequent decay of the establishments that once flourished at Worcester, Derby, and elsewhere. And we have those yet other integrations which result from the actual approximation of the similarly-occupied parts: whence result such facts as the concentration of publishers in Paternoster Row, of lawyers in the Temple and neighbourhood, of corn-merchants about Mark Lane, of civil engineers in Great George Street, of bankers in the centre of the city. Finding thus that in the evolution of the social organism, as in the evolution of individual organisms, there are integrations as well as differentiations, and moreover that these integrations are of the same three orders; we have additional reason for considering these integrations as essential parts of the developmental process, needed to be included in its formula. And further, the circumstance that in the social organism these integrations are determined by community of function, confirms the hypothesis that they are thus determined in the individual organism.

Again, we endeavoured to show deductively, that the contrasts of parts first seen in all unfolding embryos, are consequent upon the contrasted circumstances to which such parts are exposed; that thus, adaptation of constitution to conditions is the principle which determines their primary changes; and that, possibly, if we include under the formula hereditarily-transmitted adaptations, all subsequent differentiations may be similarly determined. Well, we need not long contemplate the facts to see that some of the predominant social differentiations are brought about in an analogous way. As the members of an originally-homogeneous community multiply and spread, the gradual separation into sections which simultaneously takes place, manifestly depends on differences of local circumstances. Those who happen to live near some place chosen, perhaps for its centrality, as one of periodical assemblage, become traders, and a town springs up; those who live dispersed, continue to hunt or cultivate the earth; those who spread to the sea-shore fall into maritime occupations. And each of these classes undergoes modifications of character fitting to its function. Later in the process of social evolution these local adaptations are greatly multiplied. In virtue of differences of soil and climate, the rural inhabitants in different parts of the kingdom, have their occupations partially specialized; and are respectively distinguished as chiefly producing cattle, or sheep, or wheat, or oats, or hops, or cider. People living where coal-fields are discovered become colliers; Cornishmen take to mining because Cornwall is metalliferous; and the iron-manufacture is the dominant industry where ironstone is plentiful. Liverpool has assumed the office of importing cotton, in consequence of its proximity to the district where cotton goods are made; and for analogous reasons Hull has become the chief port at which foreign wools are brought in. Even in the establishment of breweries, of dye-works, of slate-quarries, of brick-yards, we may see the same truth. So that, both in general and in detail, these industrial specializations of the social organism which characterize separate districts, primarily depend on local circumstances. Of the originally-similar units making up the social mass, different groups assume the different functions which their respective positions entail; and become adapted to their conditions. Thus, that which we concluded, *a priori*, to be the leading cause of organic differentiations, we find, *a posteriori*, to be the leading cause of social differentiations. Nay further, as we inferred that possibly the embryonic changes which are not thus directly caused, are caused by hereditarily-transmitted adaptations; so, we may actually see that in embryonic societies, such changes as are not due to direct adaptations, are in the main traceable to adaptations originally undergone by the parent society. The colonies founded by distinct nations, while they are alike in exhibiting specializations caused in the way above described, grow unlike in so far as they take on, more or less, the organizations of the nations they sprung from. A French settlement does not develop exactly after the same manner as an English one; and both assume forms different from those which Roman settlements assumed. Now the fact that the differentiation of societies is determined partly by the direct adaptation of their units to local conditions, and partly by the transmitted influence of like adaptations undergone by ancestral societies, tends strongly to enforce the conclusion, otherwise reached, that the differentiation of individual organisms, similarly results from immediate adaptations compounded with ancestral adaptations.

From confirmations thus furnished by sociology to physiology, let us now pass to a suggestion similarly furnished. A factory, or other producing establishment, or a town made up of such establishments, is an agency for elaborating some commodity consumed by society at large; and may be regarded as analogous to a gland or viscus in an individual organism. If we inquire what is the primitive mode in which one of these producing establishments grows up, we find it to be this. A single worker, who himself sells the produce of his labour, is the germ. His business increasing, he employs helpers—his sons or others; and having done this, he becomes a vendor not only of his own handiwork, but of that of others. A further increase of his business compels him to multiply his assistants, and his sale grows so rapid that he is obliged to confine himself to the process of selling: he ceases to be a producer, and becomes simply a channel through which the produce of others is conveyed to the public. Should his prosperity rise yet higher, he finds that he is unable to manage even the sale of his commodities, and has to employ others, probably of his own family, to aid him in selling; so that, to him as a main channel are now added subordinate channels. Moreover, when there grow up in one place, as a Manchester or a Birmingham, many establishments of like kind, this process is carried still further. There arise factors and buyers, who are the channels through which is transmitted the produce of many factories; and we believe that primarily these factors were manufacturers who undertook to dispose of the produce of smaller houses as well as their own, and ultimately became salesmen only. Under a converse aspect, all the stages of this development have been within these few years exemplified in our railway contractors. There are sundry men now living who illustrate the whole process in their own persons—men who were originally navvies, digging and wheeling; who then undertook some small sub-contract, and worked along with those they paid; who presently took larger contracts, and employed foremen; and who now contract for whole railways, and let portions to sub-contractors. That is to say, we have men who were originally workers, but have finally become the main channels out of which diverge secondary channels, which again bifurcate into the subordinate channels, through which flows the money (representing the nutriment) supplied by society to the actual makers of the railway. Now it seems worth inquiring whether this is not the original course followed in the evolution of secreting and excreting organs in an animal. We know that such is the process by which the liver is developed. Out of the group of bile-cells forming the germ of it, some centrally-placed ones, lying next to the intestine, are transformed into ducts through which the secretion of the peripheral bile-cells is poured into the intestine; and as the peripheral bile-cells multiply, there similarly arise secondary ducts emptying themselves into the main ones; tertiary ones into these; and so on. Recent inquiries show that the like is the case with the lungs,—that the bronchial tubes are thus formed. But while analogy suggests that this is the *original* mode in which such organs are developed, it at the same time suggests that this does not necessarily continue to be the mode. For as we find that in the social organism, manufacturing establishments are no longer commonly developed through the series of modifications above described, but now mostly arise by the direct transformation of a number of persons into master, clerks, foremen, workers, &c.; so the approximate method of forming organs, may in some cases be replaced by a direct metamorphosis of the organic units into the destined structure, without any transitional structures being passed through. That there are organs thus formed is an ascertained fact; and the additional question which analogy suggests is, whether the direct method is substituted for the indirect method.

Such parallelisms might be multiplied. And were it possible here to show in detail the close correspondence between the two kinds of organization, our case would be seen to have abundant support. But, as it is, these few illustrations will sufficiently justify the opinion that study of organized bodies may be indirectly furthered by study of the body politic. Hints may be expected, if nothing more. And thus we venture to think that the Inductive Method, usually alone employed by most physiologists, may not only derive important assistance from the Deductive Method, but may further be supplemented by the Sociological Method.

# THE NEBULAR HYPOTHESIS.

[*First published in* The Westminster Review *for July,* 1858. *In explanation of sundry passages, it seems needful to state that this essay was written in defence of the Nebular Hypothesis at a time when it had fallen into disrepute. Hence there are some opinions spoken of as current which are no longer current.*]

INQUIRING into the pedigree of an idea is not a bad means of roughly estimating its value. To have come of respectable ancestry, is *primâ facie* evidence of worth in a belief as in a person; while to be descended from a discreditable stock is, in the one case as in the other, an unfavourable index. The analogy is not a mere fancy. Beliefs, together with those who hold them, are modified little by little in successive generations; and as the modifications which successive generations of the holders undergo do not destroy the original type, but only disguise and refine it, so the accompanying alterations of belief, however much they purify, leave behind the essence of the original belief.

Considered genealogically, the received theory respecting the creation of the Solar System is unmistakably of low origin. You may clearly trace it back to primitive mythologies. Its remotest ancestor is the doctrine that the celestial bodies are personages who originally lived on the Earth—a doctrine still held by some of the negroes Livingstone visited. Science having divested the sun and planets of their divine personalities, this old idea was succeeded by the idea which even Kepler entertained, that the planets are guided in their courses by presiding spirits: no longer themselves gods, they are still severally kept in their orbits by gods. And when gravitation came to dispense with these celestial steersmen, there was begotten a belief, less gross than its parent, but partaking of the same essential nature, that the planets were originally launched into their orbits by the Creator's hand. Evidently, though much refined, the anthropomorphism of the current hypothesis is inherited from the aboriginal anthropomorphism, which described gods as a stronger order of men.

There is an antagonist hypothesis which does not propose to honour the Unknown Power manifested in the Universe, by such titles as "The Master-Builder," or "The Great Artificer;" but which regards this Unknown Power as probably working after a method quite different from that of human mechanics. And the genealogy of this hypothesis is as high as that of the other is low. It is begotten by that ever-enlarging and ever-strengthening belief in the presence of Law, which accumulated experiences have gradually produced in the human mind. From generation to generation Science has been proving uniformities of relation among phenomena which were before thought either fortuitous or supernatural in their origin—has been showing an established order and a constant causation where ignorance had assumed irregularity and arbitrariness. Each further discovery of Law has increased the presumption that Law is everywhere conformed to. And hence, among other beliefs, has arisen the belief that the Solar System originated, not by *manufacture* but by *evolution*. Besides its abstract parentage in those grand general conceptions which Science has generated, this hypothesis has a concrete parentage of the highest character. Based as it is on the law of universal gravitation, it may claim for its remote progenitor the great thinker who established that law. It was first suggested by one who ranks high among philosophers. The man who collected evidence indicating that stars result from the aggregation of diffused matter, was the most diligent, careful, and original astronomical observer of modern times. And the world has not seen a more learned mathematician than the man who, setting out with this conception of diffused matter concentrating towards its centre of gravity, pointed out the way in which there would arise, in the course of its concentration, a balanced group of sun, planets, and satellites, like that of which the Earth is a member.

Thus, even were there but little direct evidence assignable for the Nebular Hypothesis, the probability of its truth would be strong. Its own high derivation and the low derivation of the antagonist hypothesis, would together form a weighty reason for accepting it—at any rate, provisionally. But the direct evidence assignable for the Nebular Hypothesis is by no means little. It is far greater in quantity, and more varied in kind, than is commonly supposed. Much has been said here and there on this or that class of evidences; but nowhere, so far as we know, have all the evidences been fully stated. We propose here to do something towards supplying the deficiency: believing that, joined with the *a priori* reasons given above, the array of *a posteriori* reasons will leave little doubt in the mind of any candid inquirer.

And first, let us address ourselves to those recent discoveries in stellar astronomy which have been supposed to conflict with this celebrated speculation.

When Sir William Herschel, directing his great reflector to various nebulous spots, found them resolvable into clusters of stars, he inferred, and for a time maintained, that all nebulous spots are clusters of stars exceedingly remote from us. But after years of conscientious investigation, he concluded that "there were nebulosities which are not of a starry nature;" and on this conclusion was based his hypothesis of a diffused luminous fluid which, by its eventual aggregation, produced stars. A telescopic power much exceeding that used by Herschel, has enabled Lord Rosse to resolve some of the nebulæ previously unresolved; and, returning to the conclusion which Herschel first formed on similar grounds but afterwards rejected, many astronomers have assumed that, under sufficiently high powers, every nebula would be decomposed into stars—that the irresolvability is due solely to distance. The hypothesis now commonly entertained is, that all nebulæ are galaxies more or less like in nature to that immediately surrounding us; but that they are so inconceivably remote as to look, through ordinary telescopes, like small faint spots. And not a few have drawn the corollary, that by the discoveries of Lord Rosse the Nebular Hypothesis has been disproved.

Now, even supposing that these inferences respecting the distances and natures of the nebulæ are valid, they leave the Nebular Hypothesis substantially as it was. Admitting that each of these faint spots is a sidereal system, so far removed that its countless stars give less light than one small star of our own sidereal system; the admission is in no way inconsistent with the belief that stars, and their attendant planets, have been formed by the aggregation of nebulous matter. Though, doubtless, if the existence of nebulous matter now in course of concentration be disproved, one of the evidences of the Nebular Hypothesis is destroyed, yet the remaining evidences remain. It is a tenable position that though nebular condensation is now nowhere to be seen in progress, yet it was once going on universally. And, indeed, it might be argued that the still-continued existence of diffused nebulous matter is scarcely to be expected; seeing that the causes which have resulted in the aggregation of one mass, must have been acting on all masses, and that hence the existence of masses not aggregated would be a fact calling for explanation. Thus, granting the immediate conclusions suggested by these recent disclosures of the six-feet reflector, the corollary which many have drawn is inadmissible.

But these conclusions may be successfully contested.

Receiving them though we have been, for years past, as established truths, a critical examination of the facts has convinced us that they are quite unwarrantable. They involve so many manifest incongruities, that we have been astonished to find men of science entertaining them, even as probable. Let us consider these incongruities.

In the first place, mark what is inferable from the distribution of nebulæ.

> "The spaces which precede or which follow simple nebulæ," says Arago, "and *à fortiori*, groups of nebulæ, contain generally few stars. Herschel found this rule to be invariable. Thus every time that during a short interval no star approached in virtue of the diurnal motion, to place itself in the field of his motionless telescope, he was accustomed to say to the secretary who assisted him,—'Prepare to write; nebulæ are about to arrive.'"

How does this fact consist with the hypothesis that nebulæ are remote galaxies? If there were but one nebula, it would be a curious coincidence were this one nebula so placed in the distant regions of space, as to agree in direction with a starless spot in our own sidereal system. If there were but two nebulæ, and both were so placed, the coincidence would be excessively strange. What, then, shall we say on finding that there are thousands of nebulæ so placed? Shall we believe that in thousands of cases these far-removed galaxies happen to agree in their visible positions with the thin places in our own galaxy? Such a belief is impossible.

Still more manifest does the impossibility of it become when we consider the general distribution of nebulæ. Besides again showing itself in the fact that "the poorest regions in stars are near the richest in nebulæ," the law above specified applies to the heavens as a whole. In that zone of celestial space where stars are excessively abundant, nebulæ are rare; while in the two opposite celestial spaces that are furthest removed from this zone, nebulæ are abundant. Scarcely any nebulæ lie near the galactic circle (or plane of the Milky Way); and the great mass of them lie round the galactic poles. Can this also be mere coincidence? When to the fact that the general mass of nebulæ are antithetical in position to the general mass of stars, we add the fact that local regions of nebulæ are regions where stars are scarce, and the further fact that single nebulæ are habitually found in comparatively starless spots; does not the proof of a physical connexion become overwhelming? Should it not require an infinity of evidence to show that nebulæ are not parts of our sidereal system? Let us see whether any such infinity of evidence is assignable. Let us see whether there is even a single alleged proof which will bear examination.

"As seen through colossal telescopes," says Humboldt, "the contemplation of these nebulous masses leads us into regions from whence a ray of light, according to an assumption not wholly improbable, requires millions of years to reach our earth—to distances for whose measurement the dimensions (the distance of Sirius, or the calculated distances of the binary stars in Cygnus and the Centaur) of our nearest stratum of fixed stars scarcely suffice."

In this confused sentence there is implied a belief, that the distances of the nebulæ from our galaxy of stars as much transcend the distances of our stars from one another, as these interstellar distances transcend the dimensions of our planetary system. Just as the diameter of the Earth's orbit, is a mere point when compared with the distance of our Sun from Sirius; so is the distance of our Sun from Sirius, a mere point when compared with the distance of our galaxy from those far-removed galaxies constituting nebulæ. Observe the consequences of this assumption.

If one of these supposed galaxies is so remote that its distance dwarfs our interstellar spaces into points, and therefore makes the dimensions of our whole sidereal system relatively insignificant; does it not inevitably follow that the telescopic power required to resolve this remote galaxy into stars, must be incomparably greater than the telescopic power required to resolve the whole of our own galaxy into stars? Is it not certain that an instrument which can just exhibit with clearness the most distant stars of our own cluster, must be utterly unable to separate one of these remote clusters into stars? What, then, are we to think when we find that the same instrument which decomposes hosts of nebulæ into stars, *fails* to resolve completely our own Milky Way? Take a homely comparison. Suppose a man who was surrounded by a swarm of bees, extending, as they sometimes do, so high in the air as to render some of the individual bees almost invisible, were to declare that a certain spot on the horizon was a swarm of bees; and that he knew it because he could see the bees as separate specks. Incredible as the assertion would be, it would not exceed in incredibility this which we are criticising. Reduce the dimensions to figures, and the absurdity becomes still more palpable. In round numbers, the distance of Sirius from the Earth is half a million times the distance of the Earth from the Sun; and, according to the hypothesis, the distance of a nebula is something like half a million times the distance of Sirius. Now, our own "starry island, or nebula," as Humboldt calls it, "forms a lens-shaped, flattened, and everywhere detached stratum, whose major axis is estimated at seven or eight hundred, and its minor axis at a hundred and fifty times the distance of Sirius from the Earth."* And since it is concluded that the Solar System is near the centre of this aggregation, it follows that our distance from the remotest parts of it is some four hundred distances of Sirius. But the stars forming these remotest parts are not individually visible, even through telescopes of the highest power. How, then, can such telescopes make individually visible the stars of a nebula which is half a million times the distance of Sirius? The implication is, that a star rendered invisible by distance becomes visible if taken twelve hundred times further off! Shall we accept this implication? or shall we not rather conclude that the nebulæ are *not* remote galaxies? Shall we not infer that, be their nature what it may, they must be at least as near to us as the extremities of our own sidereal system?

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* *Cosmos.* (Seventh Edition.) Vol. i. pp. 79, 80.

Throughout the above argument, it is tacitly assumed that differences of apparent magnitude among the stars, result mainly from differences of distance. On this assumption the current doctrines respecting the nebulæ are founded; and this assumption is, for the nonce, admitted in each of the foregoing criticisms. From the time, however, when it was first made by Sir W. Herschel, this assumption has been purely gratuitous; and it now proves to be inadmissible. But, awkwardly enough, its truth and its untruth are alike fatal to the conclusions of those who argue after the manner of Humboldt. Note the alternatives.

On the one hand, what follows from the untruth of the assumption? If apparent largeness of stars is not due to comparative nearness, and their successively smaller sizes to their greater and greater degrees of remoteness, what becomes of the inferences respecting the dimensions of our sidereal system and the distances of nebulæ? If, as has lately been shown, the almost invisible star 61 Cygni has a greater parallax than *a* Cygni, though, according to an estimate based on Sir W. Herschel's assumption, it should be about twelve times more distant—if, as it turns out, there exist telescopic stars which are nearer to us than Sirius; of what worth is the conclusion that the nebulæ are very remote, because their component luminous masses are made visible only by high telescopic powers? Clearly, if the most brilliant star in the heavens and a star that cannot be seen by the naked eye, prove to be equidistant, relative distances cannot be in the least inferred from relative visibilities. And if so, nebulæ may be comparatively near, though the starlets of which they are made up appear extremely minute.

On the other hand, what follows if the truth of the assumption be granted? The arguments used to justify this assumption in the case of the stars, equally justify it in the case of the nebulæ. It cannot be contended that, on the average, the *apparent* sizes of the stars indicate their distances, without its being admitted that, on the average, the *apparent* sizes of the nebulæ indicate their distances—that, generally speaking, the larger are the nearer and the smaller are the more distant. Mark, now, the necessary inference respecting their resolvability. The largest or nearest nebulæ will be most easily resolved into stars; the successively smaller will be successively more difficult of resolution; and the irresolvable ones will be the smallest ones. This, however, is exactly the reverse of the fact. The largest nebulæ are either wholly irresolvable, or but partially resolvable under the highest telescopic powers; while large numbers of quite small nebulæ are easily resolved by far less powerful telescopes. An instrument through which the great nebula in Andromeda, two and a half degrees long and one degree broad, appears merely as a diffused light, decomposes a nebula of fifteen minutes diameter into twenty thousand starry points. At the same time that the individual stars of a nebula eight minutes in diameter are so clearly seen as to allow of their number being estimated, a nebula covering an area five hundred times as great shows no stars at all! What possible explanation of this can be given on the current hypothesis?

Yet a further difficulty remains—one which is, perhaps, still more obviously fatal than the foregoing. This difficulty is presented by the phenomena of the Magellanic clouds. Describing the larger of these, Sir John Herschel says:—

> "The Nubecula Major, like the Minor, consists partly of large tracts and ill-defined patches of irresolvable nebula, and of nebulosity in every stage of

resolution, up to perfectly resolved stars like the Milky Way, as also of regular and irregular nebulæ properly so called, of globular clusters in every stage of resolvability, and of clustering groups sufficiently insulated and condensed to come under the designation of 'clusters of stars.'"—*Cape Observations*, p. 146.

In his *Outlines of Astronomy*, Sir John Herschel, after repeating this description in other words, goes on to remark that—

"This combination of characters, rightly considered, is in a high degree instructive, affording an insight into the probable comparative distance of *stars* and *nebulæ*, and the real brightness of individual stars as compared with one another. Taking the apparent semidiameter of the nubecula major at three degrees, and regarding its solid form as, roughly speaking, spherical, its nearest and most remote parts differ in their distance from us by a little more than a tenth part of our distance from its center. The brightness of objects situated in its nearer portions, therefore, cannot be *much* exaggerated, nor that of its remoter *much* enfeebled, by their difference of distance; yet within this globular space, we have collected upwards of six hundred stars of the seventh, eighth, ninth, and tenth magnitudes, nearly three hundred nebulæ, and globular and other clusters, *of all degrees of resolvability*, and smaller scattered stars innumerable of every inferior magnitude, from the tenth to such as by their multitude and minuteness constitute irresolvable nebulosity, extending over tracts of many square degrees. Were there but one such object, it might be maintained without utter improbability that its apparent sphericity is only an effect of foreshortening, and that in reality a much greater proportional difference of distance between its nearer and more remote parts exists. But such an adjustment, improbable enough in one case, must be rejected as too much so for fair argument in two. It must, therefore, be taken as a demonstrated fact, that stars of the seventh or eighth magnitude and irresolvable nebula may co-exist within limits of distance not differing in proportion more than as nine to ten."—*Outlines of Astronomy* (10th Ed.), pp. 656-57.

This supplies yet another *reductio ad absurdum* of the doctrine we are combating. It gives us the choice of two incredibilities. If we are to believe that one of these included nebulæ is so remote that its hundred-thousand stars look like a milky spot, invisible to the naked eye; we must also believe that there are single stars so enormous that though removed to this same distance they remain visible. If we accept the other alternative, and say that many nebulæ are no further off than our own stars of the eighth magnitude; then it is requisite to say that at a distance not greater than that at which a single star is still faintly visible to the naked eye, there may exist a group of a hundred thousand stars which is invisible to the naked eye. Neither of these suppositions can be entertained. What, then, is the conclusion that remains? This only: —that the nebulæ are not further from us than parts of our own sidereal system, of which they must be considered members; and that when they are resolvable into discrete masses, these masses cannot be considered as stars in anything like the ordinary sense of that word.*

And now, having seen the untenability of this idea, rashly espoused by sundry astronomers, that the nebulæ are extremely remote galaxies; let us consider whether the various appearances they present are not reconcilable with the Nebular Hypothesis.

Given a rare and widely-diffused mass of nebulous matter, having a diameter, say, of one hundred times that of the Solar System,† what are the successive changes that may be expected to take place in it? Mutual gravitation will approximate its atoms or its molecules; but their approximation will be opposed by that atomic motion the resultant of which we recognize as repulsion, and the overcoming of which implies the evolution of heat. As fast as this heat partially escapes by radiation, further approximation will take place, attended by further evolution of heat, and so on continuously: the processes not occurring separately as here described, but simultaneously, uninterruptedly, and with increasing activity. When the nebulous mass has reached a particular stage of condensation—when its internally-situated atoms have approached to within certain distances, have generated a certain amount of heat, and are subject to a certain mutual pressure, combinations may be anticipated. Whether the molecules produced be of kinds such as we know, which is possible, or whether they be of kinds simpler than any we know, which is more probable, matters not to the argument. It suffices that molecular unions, either between atoms of the same kind or between atoms of different kinds, will finally take place. When they do take place, they will be accompanied by a sudden and great disengagement of heat; and until this excess of heat has escaped, the newly-formed molecules will remain uniformly diffused, or, as it were, dissolved in the pre-existing nebulous medium.

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* Since the publication of this essay the late Mr. R. A. Proctor has given various further reasons for the conclusion that the nebulæ belong to our own sidereal system. The opposite conclusion, contested throughout the foregoing section, has now been tacitly abandoned.

† Any objection made to the extreme tenuity this involves, is met by the calculation of Newton, who proved that were a spherical inch of air removed four thousand miles from the Earth, it would expand into a sphere more than filling the orbit of Saturn.

But now what may be expected by and by to happen? When radiation has adequately lowered the temperature, these molecules will precipitate; and, having precipitated, they will not remain uniformly diffused, but will aggregate into flocculi; just as water, precipitated from air, collects into clouds. Concluding, thus, that a nebulous mass will, in course of time, resolve itself into flocculi of precipitated denser matter, floating in the rarer medium from which they were precipitated, let us inquire what are the mechanical results to be inferred. Of clustered bodies in empty space, each will move along a line which is the resultant of the tractive forces exercised by all the rest, modified from moment to moment by the acquired motion; and the aggregation of such clustered bodies, if it eventually results at all, can result only from collision, dissipation, and the formation of a resisting medium. But with clustered bodies already immersed in a resisting medium, and especially if such bodies are of small densities, such as those we are considering, the process of concentration will begin forthwith: two factors conspiring to produce it. The flocculi described, irregular in their shapes and presenting, as they must in nearly all cases, unsymmetrical faces to their lines of motion, will be deflected from those courses which mutual gravitation, if uninterfered with, would produce among them; and this will militate against that balancing of movements which permanence of the cluster pre-supposes. If it be said, as it may truly be said, that this is too trifling a cause of derangement to produce much effect, then there comes the more important cause with which it co-operates. The medium from which the flocculi have been precipitated, and through which they are moving, must, by gravitation, be rendered denser in its central parts than in its peripheral parts. Hence the flocculi, none of them moving in straight lines to the common centre of gravity, but having courses made to diverge to one or other side of it (in small degrees by the cause just assigned, and in much greater degrees by the tractive forces of other flocculi) will, in moving towards the central region, meet with greater resistances on their inner sides than on their outer sides; and will be thus made to diverge outwardly from their courses more than they would otherwise do. Hence a tendency which, apart from other tendencies, will cause them severally to go on one or other side of the centre of gravity, and, approaching it, to get motions more and more tangential. Observe, however, that their respective motions will be deflected, not towards one side of the common centre of gravity, but towards various sides. How then can there result a movement common to them all? Very simply. Each flocculus, in describing its course, must give motion to the medium through which it is moving. But the probabilities are infinity to one against all the respective motions thus impressed on this medium, exactly balancing one another. And if they do not balance one another the result must be rotation of the whole mass of the medium in one direction. But preponderating momentum in one direction, having caused rotation of the medium in that direction, the rotating medium must in its turn gradually arrest such flocculi as are moving in opposition, and impress its own motion upon them; and thus there will ultimately be formed a rotating medium with suspended flocculi partaking of its motion, while they move in converging spirals towards the common centre of gravity.*

Before comparing these conclusions with facts, let us pursue the reasoning a little further, and observe certain subordinate actions. The respective flocculi must be drawn not towards their common centre of gravity only, but also towards neighbouring flocculi. Hence the whole assemblage of flocculi will break up into groups: each group concentrating towards its local centre of gravity, and in so doing acquiring a vortical movement like that subsequently acquired by the whole nebula. According to circumstances, and chiefly according to the size of the original nebulous mass, this process of local aggregation will produce various results. If the whole nebula is but small, the local groups of flocculi may be drawn into the common centre of gravity before their constituent masses have coalesced with one another. In a larger nebula, these local aggregations may have concentrated into rotating spheroids of vapour, while yet they have made but little approach towards the general focus of the system. In a still larger nebula, where the local aggregations are both greater and more remote from the common centre of gravity, they may have condensed into masses of molten matter before the general distribution of them has greatly altered. In short, as the conditions in each case determine, the discrete masses produced may vary indefinitely in number, in size, in density, in motion, in distribution.

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* A reference may fitly be made here to a reason given by Mons. Babinet for rejection of the Nebular Hypothesis. He has calculated that taking the existing Sun, with its observed angular velocity, its substance, if expanded so as to fill the orbit of Neptune, would have nothing approaching the angular velocity which the time of revolution of that planet implies. The assumption he makes is inadmissible. He supposes that all parts of the nebulous spheroid when it filled Neptune's orbit, had the same angular velocities. But the process of nebular condensation as indicated above, implies that the remoter flocculi of nebulous matter, later in reaching the central mass, and forming its peripheral portions, will acquire, during their longer journeys towards it, greater velocities. An inspection of one of the spiral nebulæ, as 51st or 99th Messier, at once shows that the outlying portions when they reach the nucleus, will form an equatorial belt moving round the common centre more rapidly than the rest. Thus the central parts will have small angular velocities, while there will be increasing angular velocities of parts increasingly remote from the centre. And while the density of the spheroid continues small, fluid friction will scarcely at all change these differences.

A like criticism may, I think, be passed on an opinion expressed by Prof. Newcomb. He says:—"When the contraction [of the nebulous spheroid] had gone so far that the centrifugal and attracting forces nearly balanced each other at the outer equatorial limit of the mass, the result would have been that contraction in the direction of the equator would cease entirely, and be confined to the polar regions, each particle dropping, not towards the sun, but towards the plane of the solar equator. Thus, we should have a constant flattening of the spheroidal atmosphere until it was reduced to a thin flat disk. This disk might then separate itself into rings, which would form planets in much the same way that Laplace supposed. But there would probably be no marked difference in the age of the planets." (*Popular Astronomy*, p. 512.) Now this conclusion assumes, like that of M. Babinet, that all parts of the nebulous spheroid had equal angular velocities. If, as above contended, it is inferable from the process by which a nebulous spheroid was formed, that its outer portions revolved with greater angular velocities than its inner; then the inference which Prof. Newcomb draws is not necessitated.

And now let us return to the visible characters of nebulæ, as observed through modern telescopes. Take first the description of those nebulæ which, by the hypothesis, must be in an early stage of evolution.

> Among the "*irregular nebulæ*," says Sir John Herschel, "may be comprehended all which, to *a want of complete and in most instances even of partial resolvability* by the power of the 20-feet reflector, unite such a deviation from the circular or elliptic form, or such a want of symmetry (with that form) as preclude their being placed in class 1, or that of Regular Nebulæ. This second class comprises many of the most remarkable and interesting objects in the heavens, *as well as the most extensive in respect of the area they occupy.*"

And, referring to this same order of objects, M. Arago says:—"The forms of very large diffuse nebulæ do not appear to admit of definition; they have no regular outline."

This coexistence of largeness, irregularity, and indefiniteness of outline, with irresolvability, is extremely significant. The fact that the largest nebulæ are either irresolvable or very difficult to resolve, might have been inferred *a priori;* seeing that irresolvability, implying that the aggregation of precipitated matter has gone on to but a small extent, will be found in nebulæ of wide diffusion. Again, the irregularity of these large, irresolvable nebulæ, might also have been expected; seeing that their outlines, compared by Arago with "the fantastic figures which characterize clouds carried away and tossed about by violent and often contrary winds," are similarly characteristic of a mass not yet gathered together by the mutual attraction of its parts. And once more, the fact that these large, irregular, irresolvable nebulæ have indefinite outlines —outlines that fade off insensibly into surrounding darkness—is one of like meaning.

Speaking generally (and of course differences of distance negative anything beyond average statements), the spiral nebulæ are smaller than the irregular nebulæ, and more resolvable; at the same time that they are not so small as the regular nebulæ, and not so resolvable. This is as, according to the hypothesis, it should be. The degree of condensation causing spiral movement, is a degree of condensation also implying masses of flocculi that are larger, and therefore more visible, than those existing in an earlier stage. Moreover, the forms of these spiral nebulæ are quite in harmony with the explanation given. The curves of luminous matter which they exhibit, are *not* such as would be described by discrete masses starting from a state of rest, and moving through a resisting medium to a common centre of gravity; but they *are* such as would be described by masses having their movements modified by the rotation of the medium.

In the centre of a spiral nebula is seen a mass both more luminous and more resolvable than the rest. Assume that, in process of time, all the spiral streaks of luminous matter which converge to this centre are drawn into it, as they must be; assume further, that the flocculi, or other discrete portions constituting these luminous streaks, aggregate into larger masses at the same time that they approach the central group, and that the masses forming this central group also aggregate into larger masses; and there will finally result a cluster of such larger masses, which will be resolvable with comparative ease. And, as the coalescence and concentration go on, the constituent masses will gradually become fewer, larger, brighter, and more densely collected around the common centre of gravity. See now how completely this inference agrees with observation. "The circular form is that which most commonly characterises resolvable nebulæ," writes Arago. Resolvable nebulæ, says Sir John Herschel, "are almost universally round or oval." Moreover, the centre of each group habitually displays a closer clustering of the constituent masses than the outer parts; and it is shown that, under the law of gravitation, which we now know extends to the stars, this distribution is *not* one of equilibrium, but implies progressing concentration. While, just as we inferred that, according to circumstances, the extent to which aggregation has been carried must vary; so we find that, in fact, there are regular nebulæ of all degrees of resolvability, from those consisting of innumerable minute masses, to those in which their numbers are smaller and the sizes greater, and to those in which there are a few large bodies worthy to be called stars.

On the one hand, then, we see that the notion, of late years uncritically received, that the nebulæ are extremely remote galaxies of stars like those which make up our own Milky Way, is totally irreconcileable with the facts—involves us in sundry absurdities. On the other hand, we see that the hypothesis of nebular condensation harmonizes with the most recent results of stellar astronomy: nay more—that it supplies us with an explanation of various appearances which in its absence would be incomprehensible.

Descending now to the Solar System, let us consider first a class of phenomena in some sort transitional—those offered by comets. In them, or at least in those most numerous of them which lie far out of the plane of the Solar System, and are not to be counted among its members, we have, still existing, a kind of matter like that out of which, according to the Nebular Hypothesis, the Solar System was evolved. Hence, for the explanation of them, we must go back to the time when the substances forming the sun and planets were yet unconcentrated.

When diffused matter, precipitated from a rarer medium, is aggregating, there are certain to be here and there produced small flocculi, which long remain detached; as do, for instance, minute shreds of cloud in a summer sky. In a concentrating nebula these will, in the majority of cases, eventually coalesce with the larger flocculi near to them. But it is tolerably evident that some of those formed at the outermost parts of the nebula, will *not* coalesce with the larger internal masses, but will slowly follow without overtaking them. The relatively greater resistance of the medium necessitates this. As a single feather falling to the ground will be rapidly left behind by a pillow-full of feathers; so, in their progress to the common centre of gravity, will the outermost shreds of vapour be left behind by the great masses of vapour internally situated. But we are not dependent merely on reasoning for this belief. Observation shows us that the less concentrated external parts of nebulæ, *are* left behind by the more concentrated internal parts. Examined through high powers, all nebulæ, even when they have assumed regular forms, are seen to be surrounded by luminous streaks, of which the directions show that they are being drawn into the general mass. Still higher powers bring into view still smaller, fainter, and more widely-dispersed streaks. And it cannot be doubted that the minute fragments which no telescopic aid makes visible, are yet more numerous and widely dispersed. Thus far, then, inference and observation are at one.

Granting that the great majority of these outlying portions of nebulous matter will be drawn into the central mass long before it reaches a definite form, the presumption is that some of the very small, far-removed portions will not be so; but that before they arrive near it, the central mass will have contracted into a comparatively moderate bulk. What now will be the characters of these late-arriving portions?

In the first place, they will have either extremely eccentric orbits or non-elliptic paths. Left behind at a time when they were moving towards the centre of gravity in slightly-deflected lines, and therefore having but very small angular velocities, they will approach the central mass in greatly elongated curves; and rushing round it, will go off again into space. That is, they will behave just as we see the majority of comets do; the orbits of which are either so eccentric as to be indistinguishable from parabolas, or else are not orbits at all, but are paths which are distinctly either parabolic or hyperbolic.

In the second place, they will come from all parts of the heavens. Our supposition implies that they were left behind at a time when the nebulous mass was of irregular shape, and had not acquired a definite rotation; and as the separation of them would not be from any one surface of the nebulous mass more than another, the conclusion must be that they will come to the central body from various directions in space. This, too, is exactly what happens. Unlike planets, whose orbits approximate to one plane, comets have orbits that show no relation to one another; but cut the plane of the ecliptic at all angles, and have axes inclined to it at all angles.

In the third place, these remotest flocculi of nebulous matter will, at the outset, be deflected from their direct courses to the common centre of gravity, not all on one side, but each on such side as its form, or its original proper motion, determines. And being left behind before the rotation of the nebula is set up, they will severally retain their different individual motions. Hence, following the concentrated mass, they will eventually go round it on all sides; and as often from right to left as from left to right. Here again the inference perfectly corresponds with the facts. While all the planets go round the sun from west to east, comets as often go round the sun from east to west as from west to east. Of 262 comets recorded since 1680, 130 are direct, and 132 are retrograde. This equality is what the law of probabilities would indicate.

Then, in the fourth place, the physical constitution of comets accords with the hypothesis.* The ability of nebulous matter to concentrate into a concrete form, depends on its mass. To bring its ultimate atoms into that proximity requisite for chemical union—requisite, that is, for the production of denser matter—their repulsion must be overcome. The only force antagonistic to their repulsion, is their mutual gravitation. That their mutual gravitation may generate a pressure and temperature of sufficient intensity, there must be an enormous accumulation of them; and even then the approximation can slowly go on only as fast as the evolved heat escapes. But where the quantity of atoms is small, and therefore the force of mutual gravitation small, there will be nothing to coerce the atoms into union. Whence we infer that these detached fragments of nebulous matter will continue in their original state. Non-periodic comets seem to do so.

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* It is true that since this essay was written reasons have been given for concluding that comets consist of swarms of meteors enveloped in aeriform matter. Very possibly this is the constitution of the periodic comets which, approximating their orbits to the plane of the Solar System, form established parts of the System, and which, as will be hereafter indicated, have probably a quite different origin.

We have already seen that this view of the origin of comets harmonizes with the characters of their orbits; but the evidence hence derived is much stronger than was indicated. The great majority of cometary orbits are classed as parabolic; and it is ordinarily inferred that they are visitors from remote space, and will never return. But are they rightly classed as parabolic? Observations on a comet moving in an extremely eccentric ellipse, which are possible only when it is comparatively near perihelion, must fail to distinguish its orbit from a parabola. Evidently, then, it is not safe to class it as a parabola because of inability to detect the elements of an ellipse. But if extreme eccentricity of an orbit necessitates such inability, it seems quite possible that comets have no other orbits than elliptic ones. Though five or six are said to be hyperbolic, yet, as I learn from one who has paid special attention to comets, "no such orbit has, I believe, been computed for a well-observed comet." Hence the probability that all the orbits are ellipses is overwhelming. Ellipses and hyperbolas have countless varieties of forms, but there is only one form of parabola; or, to speak literally, all parabolas are similar, while there are infinitely numerous dissimilar ellipses and dissimilar hyperbolas. Consequently, anything coming to the Sun from a great distance must have one exact amount of proper motion to produce a parabola: all other amounts would give hyperbolas or ellipses. And if there are no hyperbolic orbits, then it is infinity to one that all the orbits are elliptical. This is just what they would be if comets had the genesis above supposed.

And now, leaving these erratic bodies, let us turn to the more familiar and important members of the Solar System. It was the remarkable harmony among their movements which first made Laplace conceive that the Sun, planets, and satellites had resulted from a common genetic process.

As Sir William Herschel, by his observations on the nebulæ, was led to the conclusion that stars resulted from the aggregation of diffused matter; so Laplace, by his observations on the structure of the Solar System, was led to the conclusion that only by the rotation of aggregating matter were its peculiarities to be explained. In his *Exposition du Système du Monde*, he enumerates as the leading evidences:—1. The movements of the planets in the same direction and in orbits approaching to the same plane; 2. The movements of the satellites in the same direction as those of the planets; 3. The movements of rotation of these various bodies and of the sun in the same direction as the orbital motions, and mostly in planes little different; 4. The small eccentricities of the orbits of the planets and satellites, as contrasted with the great eccentricities of the cometary orbits. And the probability that these harmonious movements had a common cause, he calculates as two hundred thousand billions to one.

This immense preponderance of probability does not point to a common cause under the form ordinarily conceived—an Invisible Power working after the method of "a Great Artificer;" but to an Invisible Power working after the method of evolution. For though the supporters of the common hypothesis may argue that it was necessary for the sake of stability that the planets should go round the Sun in the same direction and nearly in one plane, they cannot thus account for the direction of the axial motions.* The mechanical equilibrium would not have been interfered with, had the Sun been without any rotatory movement; or had he revolved on his axis in a direction opposite to that in which the planets go round him; or in a direction at right angles to the average plane of their orbits. With equal safety the motion of the Moon round the Earth might have been the reverse of the Earth's motion round its axis; or the motions of Jupiter's satellites might similarly have been at variance with his axial motion; or those of Saturn's satellites with his. As, however, none of these alternatives have been followed, the uniformity must be considered, in this case as in all others, evidence of subordination to some general law—implies what we call natural causation, as distinguished from arbitrary arrangement.

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* Though this rule fails at the periphery of the Solar System, yet it fails only where the axis of rotation, instead of being almost perpendicular to the orbit-plane, is very little inclined to it; and where, therefore, the forces tending to produce the congruity of motions were but little operative.

Hence the hypothesis of evolution would be the only probable one, even in the absence of any clue to the particular mode of evolution. But when we have, propounded by a mathematician of the highest authority, a theory of this evolution based on established mechanical principles, which accounts for these various peculiarities, as well as for many minor ones, the conclusion that the Solar System *was* evolved becomes almost irresistible.

The general nature of Laplace's theory scarcely needs stating. Books of popular astronomy have familiarized most readers with his conceptions;—namely, that the matter now condensed into the Solar System, once formed a vast rotating spheroid of extreme rarity extending beyond the orbit of the outermost planet; that as this spheroid contracted, its rate of rotation necessarily increased; that by augmenting centrifugal force its equatorial zone was from time to time prevented from following any further the concentrating mass, and so remained behind as a revolving ring; that each of the revolving rings thus periodically detached, eventually became ruptured at its weakest point, and, contracting on itself, gradually aggregated into a rotating mass; that this, like the parent mass, increased in rapidity of rotation as it decreased in size, and, where the centrifugal force was sufficient, similarly left behind rings, which finally collapsed into rotating spheroids; and that thus, out of these primary and secondary rings, there arose planets and their satellites, while from the central mass there resulted the Sun. Moreover, it is tolerably well known that this *a priori* reasoning harmonizes with the results of experiment. Dr. Plateau has shown that when a mass of fluid is, as far may be, protected from the action of external forces, it will, if made to rotate with adequate velocity, form detached rings; and that these rings will break up into spheroids which turn on their axes in the same direction with the central mass. Thus, given the original nebula, which, acquiring a vortical motion in the way indicated, has at length concentrated into a vast spheroid of aeriform matter moving round its axis—given this, and mechanical principles explain the rest. The genesis of a Solar System displaying movements like those observed, may be predicted; and the reasoning on which the prediction is based is countenanced by experiment.*

But now let us inquire whether, besides these most conspicuous structural and dynamic peculiarities of the Solar System, sundry minor ones are not similarly explicable.

Take first the relation between the planes of the planetary orbits and the plane of the Sun's equator. If, when the nebulous spheroid extended beyond the orbit of Neptune, all parts of it had been revolving exactly in the same plane, or rather in parallel planes—if all its parts had had one axis; then the planes of the successive rings would have been coincident with each other and with that of the Sun's rotation. But it needs only to go back to the earlier stages of concentration, to see that there could exist no such complete uniformity of motion. The flocculi, already described as precipitated from an irregular and widely-diffused nebula, and as starting from all points to their common centre of gravity, must move not in one plane but in innumerable planes, cutting each other at all angles. The gradual establishment of a vortical motion such as we at present see indicated in the spiral nebulæ, is the gradual approach towards motion in one plane. But this plane can but slowly become decided. Flocculi not moving in this plane, but entering into the aggregation at various inclinations, will tend to perform their revolutions round its centre in their own planes; and only in course of time will their motions be partly destroyed by conflicting ones, and partly resolved into the general motion. Especially will the outermost portions of the rotating mass retain for a long time their more or less independent directions. Hence the probabilities are, that the planes of the rings first detached will differ considerably from the average plane of the mass; while the planes of those detached latest will differ from it less.

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* It is true that, as expressed by him, these propositions of Laplace are not all beyond dispute. An astronomer of the highest authority, who has favoured me with some criticisms on this essay, alleges that instead of a nebulous ring rupturing at one point, and collapsing into a single mass, "all probability would be in favour of its breaking up into many masses." This alternative result certainly seems the more likely. But granting that a nebulous ring would break up into many masses, it may still be contended that, since the chances are infinity to one against these being of equal sizes *and* equidistant, they could not remain evenly distributed round their orbit. This annular chain of gaseous masses would break up into groups of masses; these groups would eventually aggregate into larger groups; and the final result would be the formation of a single mass. I have put the question to an astronomer scarcely second in authority to the one above referred to, and he agrees that this would probably be the process.

Here, again, inference to a considerable extent agrees with observation. Though the progression is irregular, yet, on the average, the inclinations decrease on approaching the Sun; and this is all we can expect. For as the portions of the nebulous spheroid must have arrived with miscellaneous inclinations, its strata must have had planes of rotation diverging from the average plane in degrees not always proportionate to their distances from the centre.

Consider next the movements of the planets on their axes. Laplace alleged as one among other evidences of a common genetic cause, that the planets rotate in a direction the same as that in which they go round the Sun, and on axes approximately perpendicular to their orbits. Since he wrote, an exception to this general rule has been discovered in the case of Uranus, and another still more recently in the case of Neptune—judging, at least, from the motions of their respective satellites. This anomaly has been thought to throw considerable doubt on his speculation; and at first sight it does so. But a little reflection shows that the anomaly is not inexplicable, and that Laplace simply went too far in putting down as a certain result of nebular genesis, what is, in some instances, only a probable result. The cause he pointed out as determining the direction of rotation, is the greater absolute velocity of the outer part of the detached ring. But there are conditions under which this difference of velocity may be too insignificant, even if it exists. If a mass of nebulous matter approaching spirally to the central spheroid, and eventually joining it tangentially, is made up of parts having the same absolute velocities; then, after joining the equatorial periphery of the spheroid and being made to rotate with it, the angular velocity of its outer parts will be smaller than the angular velocity of its inner parts. Hence, if, when the angular velocities of the outer and inner parts of a detached ring are the same, there results a tendency to rotation in the same direction with the orbital motion, it may be inferred that when the outer parts of the ring have a smaller angular velocity than the inner parts, a tendency to retrograde rotation will be the consequence.

Again, the sectional form of the ring is a circumstance of moment; and this form must have differed more or less in every case. To make this clear, some illustration will be necessary. Suppose we take an orange, and, assuming the marks of the stalk and the calyx to represent the poles, cut off round the line of the equator a strip of peel. This strip of peel, if placed on the table with its ends meeting, will make a ring shaped like the hoop of a barrel—a ring of which the thickness in the line of its diameter is very small, but of which the width in a direction perpendicular to its diameter is considerable. Suppose, now, that in place of an orange, which is a spheroid of very slight oblateness, we take a spheroid of very great oblateness, shaped somewhat like a lens of small convexity. If from the edge or equator of this lens-shaped spheroid, a ring of moderate size were cut off, it would be unlike the previous ring in this respect, that its greatest thickness would be in the line of its diameter, and not in a line at right angles to its diameter: it would be a ring shaped somewhat like a quoit, only far more slender. That is to say, according to the oblateness of a rotating spheroid, the detached ring may be either a hoop-shaped ring or a quoit-shaped ring.

One further implication must be noted. In a much-flattened or lens-shaped spheroid, the form of the ring will vary with its bulk. A very slender ring, taking off just the equatorial surface, will be hoop-shaped; while a tolerably massive ring, trenching appreciably on the diameter of the spheroid, will be quoit-shaped. Thus, then, according to the oblateness of the spheroid and the bulkiness of the detached ring, will the greatest thickness of that ring be in the direction of its plane, or in a direction perpendicular to its plane. But this circumstance must greatly affect the rotation of the resulting planet. In a decidedly hoop-shaped nebulous ring, the differences of velocity between the inner and outer surfaces will be small; and such a ring, aggregating into a mass of which the greatest diameter is at right angles to the plane of the orbit, will almost certainly give to this mass a predominant tendency to rotate in a direction at right angles to the plane of the orbit. Where the ring is but little hoop-shaped, and the difference between the inner and outer velocities greater, as it must be, the opposing tendencies—one to produce rotation in the plane of the orbit, and the other, rotation perpendicular to it—will both be influential; and an intermediate plane of rotation will be taken up. While, if the nebulous ring is *decidedly* quoit-shaped, and therefore aggregates into a mass whose greatest dimension lies in the plane of the orbit, both tendencies will conspire to produce rotation in that plane.

On referring to the facts, we find them, as far as can be judged, in harmony with this view. Considering the enormous circumference of Uranus's orbit, and his comparatively small mass, we may conclude that the ring from which he resulted was a comparatively slender, and therefore a hoop-shaped one: especially as the nebulous mass must have been at that time less oblate than afterwards. Hence, a plane of rotation nearly perpendicular to his orbit, and a direction of rotation having no reference to his orbital movement. Saturn has a mass seven times as great, and an orbit of less than half the diameter; whence it follows that his genetic ring, having less than half the circumference, and less than half the vertical thickness (the spheroid being then certainly *as* oblate, and indeed *more* oblate), must have had a much greater width—must have been less hoop-shaped, and more approaching to the quoit-shaped: notwithstanding difference of density, it must have been at least two or three times as broad in the line of its plane. Consequently, Saturn has a rotatory movement in the same direction as the movement of translation, and in a plane differing from it by thirty degrees only. In the case of Jupiter, again, whose mass is three and a half times that of Saturn, and whose orbit is little more than half the size, the genetic ring must, for the like reasons, have been still broader—decidedly quoit-shaped, we may say; and there hence resulted a planet whose plane of rotation differs from that of his orbit by scarcely more than three degrees. Once more, considering the comparative insignificance of Mars, Earth, Venus, and Mercury, it follows that, the diminishing circumferences of the rings not sufficing to account for the smallness of the resulting masses, the rings must have been slender ones—must have again approximated to the hoop-shaped; and thus it happens that the planes of rotation again diverge more or less widely from those of the orbits. Taking into account the increasing oblateness of the original spheroid in the successive stages of its concentration, and the different proportions of the detached rings, it may fairly be held that the respective rotatory motions are not at variance with the hypothesis but contrariwise tend to confirm it.

Not only the directions, but also the velocities of rotation seem thus explicable. It might naturally be supposed that the large planets would revolve on their axes more slowly than the small ones: our terrestrial experiences of big and little bodies incline us to expect this. It is a corollary from the Nebular Hypothesis, however, more especially when interpreted as above, that while large planets will rotate rapidly, small ones will rotate slowly; and we find that in fact they do so. Other things equal, a concentrating nebulous mass which is diffused through a wide space, and whose outer parts have, therefore, to travel from great distances to the common centre of gravity, will acquire a high axial velocity in course of its aggregation; and conversely with a small mass. Still more marked will be the difference where the form of the genetic ring conspires to increase the rate of rotation. Other things equal, a genetic ring which is broadest in the direction of its plane will produce a mass rotating faster than one which is broadest at right angles to its plane; and if the ring is absolutely as well as relatively broad, the rotation will be very rapid. These conditions were, as we saw, fulfilled in the case of Jupiter; and Jupiter turns round his axis in less than ten hours. Saturn, in whose case, as above explained, the conditions were less favourable to rapid rotation, takes nearly ten hours and a half. While Mars, Earth, Venus, and Mercury, whose rings must have been slender, take more than double that time: the smallest taking the longest.

From the planets let us now pass to the satellites. *Here*, beyond the conspicuous facts commonly adverted to, that they go round their primaries in the directions in which these turn on their axes, in planes diverging but little from their equators, and in orbits nearly circular, there are several significant traits which must not be passed over.

One of them is that each set of satellites repeats in miniature the relations of the planets to the Sun, both in certain respects above named and in the order of their sizes. On progressing from the outside of the Solar System to its centre, we see that there are four large external planets, and four internal ones which are comparatively small. A like contrast holds between the outer and inner satellites in every case. Among the four satellites of Jupiter, the parallel is maintained as well as the comparative smallness of the number allows: the two outer ones are the largest, and the two inner ones the smallest. According to the most recent observations made by Mr. Lassell, the like is true of the four satellites of Uranus. In the case of Saturn, who has eight secondary planets revolving round him, the likeness is still more close in arrangement as in number: the three outer satellites are large, the inner ones small; and the contrasts of size are here much greater between the largest, which is nearly as big as Mars, and the smallest, which is with difficulty discovered even by the best telescopes. But the analogy does not end here. Just as with the planets, there is at first a general increase of size on travelling inwards from Neptune and Uranus, which do not differ very widely, to Saturn, which is much larger, and to Jupiter, which is the largest; so of the eight satellites of Saturn, the largest is not the outermost, but the outermost save two; so of Jupiter's four secondaries, the largest is the most remote but one. Now these parallelisms are inexplicable by the theory of final causes. For purposes of lighting, if this be the presumed object of these attendant bodies, it would have been far better had the larger been the nearer: at present, their remoteness renders them of less service than the smallest.

To the Nebular Hypothesis, however, these analogies give further support. They show the action of a common physical cause. They imply a *law* of genesis, holding in the secondary systems as in the primary system.

Still more instructive shall we find the distribution of the satellites—their absence in some instances, and their presence in other instances, in smaller or greater numbers. The argument from design fails to account for this distribution. Supposing it be granted that planets nearer the Sun than ourselves, have no need of moons (though, considering that their nights are as dark, and, relatively to their brilliant days, even darker than ours, the need seems quite as great)—supposing this to be granted; how are we to explain the fact that Uranus has but half as many moons as Saturn, though he is at double the distance? While, however, the current presumption is untenable, the Nebular Hypothesis furnishes us with an explanation. It enables us to predict where satellites will be abundant and where they will be absent. The reasoning is as follows.

In a rotating nebulous spheroid which is concentrating into a planet, there are at work two antagonist mechanical tendencies—the centripetal and the centrifugal. While the force of gravitation draws all the atoms of the spheroid together, their tangential momentum is resolvable into two parts, of which one resists gravitation. The ratio which this centrifugal force bears to gravitation, varies, other things equal, as the square of the velocity. Hence, the aggregation of a rotating nebulous spheroid will be more or less hindered by this resisting force, according as the rate of rotation is high or low: the opposition, in equal spheroids, being four times as great when the rotation is twice as rapid; nine times as great when it is three times as rapid; and so on. Now the detachment of a ring from a planet-forming body of nebulous matter, implies that at its equatorial zone the increasing centrifugal force consequent on concentration has become so great as to balance gravity. Whence it is tolerably obvious that the detachment of rings will be most frequent from those masses in which the centrifugal tendency bears the greatest ratio to the gravitative tendency. Though it is not possible to calculate what ratio these two tendencies had to each other in the genetic spheroid which produced each planet, it is possible to calculate where each was the greatest and where the least. While it is true that the ratio which centrifugal force now bears to gravity at the equator of each planet, differs widely from that which it bore during the earlier stages of concentration; and while it is true that this change in the ratio, depending on the degree of contraction each planet has undergone, has in no two cases been the same; yet we may fairly conclude that where the ratio is still the greatest, it has been the greatest from the beginning. The satellite-forming tendency which each planet had, will be approximately indicated by the proportion now existing in it between the aggregating power, and the power that has opposed aggregation. On making the requisite calculations, a remarkable harmony with this inference comes out. The following table shows what fraction the centrifugal force is of the centripetal force in every case; and the relation which that fraction bears to the number of satellites.*

| Mercury. | Venus. | Earth. | Mars. | Jupiter. | Saturn. | Uranus. |
|---|---|---|---|---|---|---|
| $\frac{1}{360}$ | $\frac{1}{253}$ | $\frac{1}{289}$ | $\frac{1}{127}$ | $\frac{1}{11{\cdot}4}$ | $\frac{1}{6{\cdot}4}$ | $\frac{1}{10{\cdot}9}$ |
| | | 1<br>Satellite. | 2<br>Satellites. | 4<br>Satellites. | 8<br>Satellites, and three rings. | 4<br>Satellites. |

Thus taking as our standard of comparison the Earth with its one moon, we see that Mercury, in which the centrifugal force is relatively less, has no moon. Mars, in which it is relatively much greater, has two moons. Jupiter, in which it is far greater, has four moons. Uranus, in which it is greater still, has certainly four, and more if Herschel was right. Saturn, in which it is the greatest, being nearly one-sixth of gravity, has, including his rings, eleven attendants. The only instance in which there is nonconformity with observation, is that of Venus. Here it appears that the centrifugal force is relatively greater than in the Earth; and, according to the hypothesis, Venus ought to have a satellite. Respecting this anomaly several remarks are to be made. Without putting any faith in the alleged discovery of a satellite of Venus (repeated at intervals by five different observers), it may yet be contended that as the satellites of Mars eluded

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* The comparative statement here given differs, slightly in most cases

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and in one case largely, from the statement included in this essay as originally published in 1858. As then given the table ran thus :—

| Mercury. | Venus. | Earth. | Mars. | Jupiter. | Saturn. | Uranus. |
|---|---|---|---|---|---|---|
| $\frac{1}{362}$ | $\frac{1}{282}$ | $\frac{1}{289}$ | $\frac{1}{326}$ | $\frac{1}{14}$ | $\frac{1}{6\cdot2}$ | $\frac{1}{9}$ |
| | | 1 Satellite. | | 4 Satellites. | 8 Satellites, and three rings. | 4 (or 6 according to Herschel). |

The calculations ending with these figures were made while the Sun's distance was still estimated at 95 millions of miles. Of course the reduction afterwards established in the estimated distance, entailing, as it did, changes in the factors which entered into the calculations, affected the results; and, though it was unlikely that the relations stated would be materially changed, it was needful to have the calculations made afresh. Mr. Lynn has been good enough to undertake this task, and the figures given in the text are his. In the case of Mars a large error in my calculation had arisen from accepting Arago's statement of his density (0·95), which proves to be some thing like double what it should be. Here a curious incident may be named. When, in 1877, it was discovered that Mars has two satellites, though, according to my hypothesis, it seemed that he should have none, my faith in it received a shock; and since that time I have occasionally considered whether the fact is in any way reconcilable with the hypothesis. But now the proof afforded by Mr. Lynn that my calculation contained a wrong factor, disposes of the difficulty—nay, changes the objection to a verification. It turns out that, according to the hypothesis, Mars *ought* to have satellites; and, further, that he ought to have a number intermediate between 1 and 4.

observation up to 1877, a satellite of Venus may have eluded observation up to the present time. Merely naming this as possible, but not probable, a consideration of more weight is that the period of rotation of Venus is but indefinitely fixed, and that a small diminution in the estimated angular velocity of her equator would bring the result into congruity with the hypothesis. Further, it may be remarked that not exact, but only general, congruity is to be expected; since the process of condensation of each planet from nebulous matter can scarcely be expected to have gone on with absolute uniformity: the angular velocities of the superposed strata of nebulous matter probably differed from one another in degrees unlike in each case; and such differences would affect the satellite-forming tendency. But without making much of these possible explanations of the discrepancy, the correspondence between inference and fact which we find in so many planets, may be held to afford strong support to the Nebular Hypothesis.

Certain more special peculiarities of the satellites must be mentioned as suggestive. One of them is the relation between the period of revolution and that of rotation. No discoverable purpose is served by making the Moon go round its axis in the same time that it goes round the Earth: for our convenience, a more rapid axial motion would have been equally good; and for any possible inhabitants of the Moon, much better. Against the alternative supposition, that the equality occurred by accident, the probabilities are, as Laplace says, infinity to one. But to this arrangement, which is explicable neither as the result of design nor of chance, the Nebular Hypothesis furnishes a clue. In his *Exposition du Système du Monde,* Laplace shows, by reasoning too detailed to be here repeated, that under the circumstances such a relation of movements would be likely to establish itself.

Among Jupiter's satellites, which severally display these same synchronous movements, there also exists a still more remarkable relation. "If the mean angular velocity of the first satellite be added to twice that of the third, the sum will be equal to three times that of the second;" and "from this it results that the situations of any two of them being given, that of the third can be found." Now here, as before, no conceivable advantage results. Neither in this case can the connexion have been accidental: the probabilities are infinity to one to the contrary. But again, according to Laplace, the Nebular Hypothesis supplies a solution. Are not these significant facts?

Most significant fact of all, however, is that presented by the rings of Saturn. As Laplace remarks, they are, as it were, still extant witnesses of the genetic process he propounded. Here we have, continuing permanently, forms of aggregation like those through which each planet and satellite once passed; and their movements are just what, in conformity with the hypothesis, they should be. "La durée de la rotation d'une planète doit donc être, d'après cette hypothèse, plus petite que la durée de la révolution du corps le plus voisin qui circule autour d'elle," says Laplace. And he then points out that the time of Saturn's rotation is to that of his rings as 427 to 438—an amount of difference such as was to be expected.*

Respecting Saturn's rings it may be further remarked that the place of their occurrence is not without significance.

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* Since this paragraph was first published, the discovery that Mars has two satellites revolving round him in periods shorter than that of his rotation, has shown that the implication on which Laplace here insists is general only, and not absolute. Were it a necessary assumption that all parts of a concentrating nebulous spheroid revolve with the same angular velocities, the exception would appear an inexplicable one; but if, as suggested in a preceding section, it is inferable from the process of formation of a nebulous spheroid, that its outer strata will move round the general axis with higher angular velocities than the inner ones, there follows a possible interpretation. Though, during the earlier stages of concentration, while the nebulous matter, and especially its peripheral portions, are very rare, the effects of fluid-friction will be too small to change greatly such differences of angular

Rings detached early in the process of concentration, consisting of gaseous matter having extremely little power of cohesion, can have little ability to resist the disruptive forces due to imperfect balance; and, therefore, collapse into satellites. A ring of a denser kind, whether solid, liquid, or composed of small discrete masses (as Saturn's rings are now concluded to be), we can expect will be formed only near the body of a planet when it has reached so late a stage of concentration that its equatorial portions contain matters capable of easy precipitation into liquid and, finally, solid forms. Even then it can be produced only under special conditions. Gaining a rapidly-increasing preponderance as the gravitative force does during the closing stages of concentration, the centrifugal force cannot, in ordinary cases, cause the leaving behind of rings when the mass has become dense. Only where the centrifugal force has all along been very great, and remains powerful to the last, as in Saturn, can we expect dense rings to be formed.

We find, then, that besides those most conspicuous peculiarities of the Solar System which first suggested the theory of its evolution, there are many minor ones pointing in the same direction. Were there no other evidence, these mechanical arrangements would, considered in their totality, go far to establish the Nebular Hypothesis.

From the mechanical arrangements of the Solar System, turn we now to its physical characters; and, first, let us consider the inferences deducible from relative specific gravities.

velocities as exist; yet, when concentration has reached its last stages, and the matter is passing from the gaseous into the liquid and solid states, and when also the convection-currents have become common to the whole mass (which they probably at first are not), the angular velocity of the peripheral portion will gradually be assimilated to that of the interior; and it becomes comprehensible that in the case of Mars the peripheral portion, more and more dragged back by the internal mass, lost part of its velocity during the interval between the formation of the innermost satellite and the arrival at the final form.

The fact that, speaking generally, the denser planets are the nearer to the Sun, has been by some considered as adding another to the many indications of nebular origin. Legitimately assuming that the outermost parts of a rotating nebulous spheroid, in its earlier stages of concentration, must be comparatively rare; and that the increasing density which the whole mass acquires as it contracts, must hold of the outermost parts as well as the rest; it is argued that the rings successively detached will be more and more dense, and will form planets of higher and higher specific gravities. But passing over other objections, this explanation is quite inadequate to account for the facts. Using the Earth as a standard of comparison, the relative densities run thus:—

| Neptune. | Uranus. | Saturn. | Jupiter. | Mars. | Earth. | Venus. | Mercury. | Sun. |
|---|---|---|---|---|---|---|---|---|
| 0·17 | 0·25 | 0·11 | 0·23 | 0·45 | 1·00 | 0·92 | 1·26 | 0·25 |

Two insurmountable objections are presented by this series. The first is, that the progression is but a broken one. Neptune is denser than Saturn, which, by the hypothesis, it ought not to be. Uranus is denser than Jupiter, which it ought not to be. Uranus is denser than Saturn, and the Earth is denser than Venus—facts which not only give no countenance to, but directly contradict, the alleged explanation. The second objection, still more manifestly fatal, is the low specific gravity of the Sun. If, when the matter of the Sun filled the orbit of Mercury, its state of aggregation was such that the detached ring formed a planet having a specific gravity equal to that of iron; then the Sun itself, now that it has concentrated, should have a specific gravity much greater than that of iron; whereas its specific gravity is only half as much again as that of water. Instead of being far denser than the nearest planet, it is but one-fifth as dense.

While these anomalies render untenable the position that the relative specific gravities of the planets are direct indications of nebular condensation; it by no means follows that they negative it. Several causes may be assigned for these unlikenesses:—1. Differences among the planets in respect of the elementary substances composing them; or in the proportions of such elementary substances, if they contain the same kinds. 2. Differences among them in respect of the quantities of matter they contain; for, other things equal, the mutual gravitation of molecules will make a larger mass denser than a smaller. 3. Differences of temperatures; for, other things equal, those having higher temperatures will have lower specific gravities. 4. Differences of physical states, as being gaseous, liquid, or solid; or, otherwise, differences in the relative amounts of the solid, liquid, and gaseous matter they contain.

It is quite possible, and we may indeed say probable, that all these causes come into play, and that they take various shares in the production of the several results. But difficulties stand in the way of definite conclusions. Nevertheless, if we revert to the hypothesis of nebular genesis, we are furnished with partial explanations if nothing more.

In the cooling of celestial bodies several factors are concerned. The first and simplest is the one illustrated at every fire-side by the rapid blackening of little cinders which fall into the ashes, in contrast with the long-continued redness of big lumps. This factor is the relation between increase of surface and increase of content: surfaces, in similar bodies, increasing as the squares of the dimensions while contents increase as their cubes. Hence, on comparing the Earth with Jupiter, whose diameter is about eleven times that of the Earth, it results that while his surface is 125 times as great, his content is 1390 times as great. Now even (supposing we assume like temperatures and like densities) if the only effect were that through a given area of surface eleven times more matter had to be cooled in the one case than in the other, there would be a vast difference between the times occupied in concentration. But, in virtue of a second factor, the difference would be much greater than that consequent on these geometrical relations. The escape of heat from a cooling mass is effected by conduction, or by convection, or by both. In a solid it is wholly by conduction; in a liquid or gas the chief part is played by convection—by circulating currents which continually transpose the hotter and cooler parts. Now in fluid spheroids—gaseous, or liquid, or mixed—increasing size entails an increasing obstacle to cooling, consequent on the increasing distances to be travelled by the circulating currents. Of course the relation is not a simple one: the velocities of the currents will be unlike. It is manifest, however, that in a sphere of eleven times the diameter, the transit of matter from centre to surface and back from surface to centre, will take a much longer time; even if its movement is unrestrained. But its movement is, in such cases as we are considering, greatly restrained. In a rotating spheroid there come into play retarding forces augmenting with the velocity of rotation. In such a spheroid the respective portions of matter (supposing them equal in their angular velocities round the axis, which they will tend more and more to become as the density increases), must vary in their absolute velocities according to their distances from the axis; and each portion cannot have its distance from the axis changed by circulating currents, which it must continually be, without loss or gain in its quantity of motion: through the medium of fluid friction, force must be expended, now in increasing its motion and now in retarding its motion. Hence, when the larger spheroid has also a higher velocity of rotation, the relative slowness of the circulating currents, and the consequent retardation of cooling, must be much greater than is implied by the extra distances to be travelled.

And now observe the correspondence between inference and fact. In the first place, if we compare the group of the great planets, Jupiter, Saturn, and Uranus, with the group of the small planets, Mars, Earth, Venus, and Mercury, we see that low density goes along with great size and great velocity of rotation, and that high density goes along with small size and small velocity of rotation. In the second place, we are shown this relation still more clearly if we compare the extreme instances—Saturn and Mercury. The special contrast of these two, like the general contrast of the groups, points to the truth that low density, like the satellite-forming tendency, is associated with the ratio borne by centrifugal force to gravity; for in the case of Saturn with his many satellites and least density, centrifugal force at the equator is nearly $\frac{1}{6}$th of gravity, whereas in Mercury with no satellite and greatest density centrifugal force is but $\frac{1}{360}$th of gravity.

There are, however, certain factors which, working in an opposite way, qualify and complicate these effects. Other things equal, mutual gravitation among the parts of a large mass will cause a greater evolution of heat than is similarly caused in a small mass; and the resulting difference of temperature will tend to produce more rapid dissipation of heat. To this must be added the greater velocity of the circulating currents which the intenser forces at work in larger spheroids will produce—a contrast made still greater by the relatively smaller retardation by friction to which the more voluminous currents are exposed. In these causes, joined with causes previously indicated, we may recognize a probable explanation of the otherwise anomalous fact that the Sun, though having a thousand times the mass of Jupiter, has yet reached as advanced a stage of concentration. For the force of gravity in the Sun, which at his surface is some ten times that at the surface of Jupiter, must expose his central parts to a pressure relatively very intense; producing, during contraction, a relatively rapid genesis of heat. And it is further to be remarked that, though the circulating currents in the Sun have far greater distances to travel, yet since his rotation is relatively so slow that the angular velocity of his substance is but about one-sixtieth of that of Jupiter's substance, the resulting obstacle to circulating currents is relatively small, and the escape of heat far less retarded. Here, too, we may note that in the co-operation of these factors, there seems a reason for the greater concentration reached by Jupiter than by Saturn, though Saturn is the elder as well as the smaller of the two; for at the same time that the gravitative force in Jupiter is more than twice as great as in Saturn, his velocity of rotation is very little greater, so that the opposition of the centrifugal force to the centripetal is not much more than half.

But now, not judging more than roughly of the effects of these several factors, co-operating in various ways and degrees, some to aid concentration and others to resist it, it is sufficiently manifest that, other things equal, the larger nebulous spheroids, longer in losing their heat, will more slowly reach high specific gravities; and that where the contrasts in size are so immense as those between the greater and the smaller planets, the smaller may have reached relatively high specific gravities when the greater have reached but relatively low ones. Further, it appears that such qualification of the process as results from the more rapid genesis of heat in the larger masses, will be countervailed where high velocity of rotation greatly impedes the circulating currents. Thus interpreted then, the various specific gravities of the planets may be held to furnish further evidences supporting the Nebular Hypothesis.

Increase of density and escape of heat are correlated phenomena, and hence in the foregoing section, treating of the respective densities of the celestial bodies in connexion with nebular condensation, much has been said and implied respecting the accompanying genesis and dissipation of heat. Quite apart, however, from the foregoing arguments and inferences, there is to be noted the fact that in the present temperatures of the celestial bodies at large we find additional supports to the hypothesis; and these, too, of the most substantial character. For if, as is implied above, heat must inevitably be generated by the aggregation of diffused matter, we ought to find in all the heavenly bodies, either present high temperatures or marks of past high temperatures. This we do, in the places and in the degrees which the hypothesis requires.

Observations showing that as we descend below the Earth's surface there is a progressive increase of heat, joined with the conspicuous evidence furnished by volcanoes, necessitate the conclusion that the temperature is very high at great depths. Whether, as some believe, the interior of the Earth is still molten, or whether, as Sir William Thomson contends, it must be solid; there is agreement in the inference that its heat is intense. And it has been further shown that the rate at which the temperature increases on descending below the surface, is such as would be found in a mass which had been cooling for an indefinite period. The Moon, too, shows us, by its corrugations and its conspicuous extinct volcanoes, that in it there has been a process of refrigeration and contraction, like that which has gone on in the Earth. There is no teleological explanation of these facts. The frequent destructions of life by earthquakes and volcanoes, imply, rather, that it would have been better had the Earth been created with a low internal temperature. But if we contemplate the facts in connexion with the Nebular Hypothesis, we see that this still-continued high internal heat is one of its corollaries. The Earth must have passed through the gaseous and the molten conditions before it became solid, and must for an almost infinite period by its internal heat continue to bear evidence of this origin.

The group of giant planets furnishes remarkable evidence. The *a priori* inference drawn above, that great size joined with relatively high ratio of centrifugal force to gravity must greatly retard aggregation, and must thus, by checking the genesis and dissipation of heat, make the process of cooling a slow one, has of late years received verifications from inferences drawn *a posteriori;* so that now the current conclusion among astronomers is that in physical condition the great planets are in stages midway between that of the Earth and that of the Sun. The fact that the centre of Jupiter's disc is twice or thrice as bright as his periphery, joined with the facts that he seems to radiate more light than is accounted for by reflection of the Sun's rays, and that his spectrum shows the "red-star line", are taken as evidences of luminosity; while the immense and rapid perturbations in his atmosphere, far greater than could be caused by heat received from the Sun, as well as the formation of spots analogous to those of the Sun, which also, like those of the Sun, show a higher rate of rotation near the equator than further from it, are held to imply high internal temperature. Thus in Jupiter, as also in Saturn, we find states which, not admitting of any teleological explanations (for they manifestly exclude the possibility of life), admit of explanations derived from the Nebular Hypothesis.

But the argument from temperature does not end here. There remains to be noticed a more conspicuous and still more significant fact. If the Solar System was produced by the concentration of diffused matter, which evolved heat while gravitating into its present dense form; then there is an obvious implication. Other things equal, the latest-formed mass will be the latest in cooling—will, for an almost infinite time, possess a greater heat than the earlier-formed ones. Other things equal, the largest mass will, because of its superior aggregative force, become hotter than the others, and radiate more intensely. Other things equal, the largest mass, notwithstanding the higher temperature it reaches, will, in consequence of its relatively small surface, be the slowest in losing its evolved heat. And hence, if there is one mass which was not only formed after the rest, but exceeds them enormously in size, it follows that this one will reach an intensity of incandescence far beyond that reached by the rest; and will continue in a state of intense incandescence long after the rest have cooled. Such a mass we have in the Sun. It is a corollary from the Nebular Hypothesis, that the matter forming the Sun assumed its present integrated shape at a period much more recent than that at which the planets became definite bodies. The quantity of matter contained in the Sun is nearly five million times that contained in the smallest planet, and above a thousand times that contained in the largest. And while, from the enormous gravitative force of his parts to their common centre, the evolution of heat has been intense, the facilities of radiation have been relatively small. Hence the still-continued high temperature. Just that condition of the central body which is a necessary inference from the Nebular Hypothesis, we find actually existing in the Sun.

[The paragraph which here follows, though it contains some questionable propositions, I reproduce just as it stood when first published in 1858, for reasons which will presently be apparent.]

It may be well to consider more closely, what is the probable condition of the Sun's surface. Round the globe of incandescent molten substances, thus conceived to form the visible body of the Sun [which in conformity with the argument in a previous section, now transferred to the Addenda, was inferred to be hollow and filled with gaseous matter at high tension] there is known to exist a voluminous atmosphere: the inferior brilliancy of the Sun's border, and the appearances during a total eclipse, alike show this. What now must be the constitution of this atmosphere? At a temperature approaching a thousand times that of molten iron, which is the calculated temperature of the solar surface, very many, if not all, of the substances we know as solid, would become gaseous; and though the Sun's enormous attractive force must be a powerful check on this tendency to assume the form of vapour, yet it cannot be questioned that if the body of the Sun consists of molten substances, some of them must be constantly undergoing evaporation. That the dense gases thus continually being generated will form the entire mass of the solar atmosphere, is not probable. If anything is to be inferred, either from the Nebular Hypothesis, or from the analogies supplied by the planets, it must be concluded that the outermost part of the solar atmosphere consists of what are called permanent gases—gases that are not condensible into fluid even at low temperatures. If we consider what must have been the state of things here, when the surface of the Earth was molten, we shall see that round the still molten surface of the Sun, there probably exists a stratum of dense aeriform matter, made up of sublimed metals and metallic compounds, and above this a stratum of comparatively rare medium analogous to air. What now will happen with these two strata? Did they both consist of permanent gases, they could not remain separate: according to a well-known law, they would eventually form a homogeneous mixture. But this will by no means happen when the lower stratum consists of matters that are gaseous only at excessively high temperatures. Given off from a molten surface, ascending, expanding, and cooling, these will presently reach a limit of elevation above which they cannot exist as vapour, but must condense and precipitate. Meanwhile the upper stratum, habitually charged with its quantum of these denser matters, as our air with its quantum of water, and ready to deposit them on any depression of temperature, must be habitually unable to take up any more of the lower stratum; and therefore this lower stratum will remain quite distinct from it.*

Considered in their *ensemble*, the several groups of evidences assigned amount almost to proof. We have seen that, when critically examined, the speculations of late years current respecting the nature of the nebulæ, commit their promulgators to sundry absurdities; while, on the other hand, we see that the various appearances these nebulæ present, are explicable as different stages in the precipitation and aggregation of diffused matter. We find that the immense majority of comets (*i.e.* omitting the periodic ones), by their physical constitution, their immensely-extended and variously-directed paths, the distribution of those paths, and their manifest structural relation to the Solar System, bear testimony to the past existence of that system in a nebulous form. Not only do those obvious peculiarities in the motions of the planets which first suggested the Nebular Hypothesis, supply proofs of it, but on closer examination we discover, in the slightly-diverging inclinations of their orbits, in their various rates of rotation, and their differently-directed axes of rotation, that the planets yield us yet further testimony; while the satellites,

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* I was about to suppress part of the above paragraph, written before the

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science of solar physics had taken shape, because of certain physical difficulties which stand in the way of its argument, when, on looking into recent astronomical works, I found that the hypothesis it sets forth respecting the Sun's structure has kinships to the several hypotheses since set forth by Zöllner, Faye, and Young. I have therefore decided to let it stand as it originally did.

The contemplated partial suppression just named, was prompted by recognition of the truth that to effect mechanical stability the gaseous interior of the Sun must have a density at least equal to that of the molten shell (greater, indeed, at the centre); and this seems to imply a specific gravity higher than that which he possesses. It may, indeed, be that the unknown elements which spectrum analysis shows to exist in the Sun, are metals of very low specific gravities, and that, existing in large proportion with other of the lighter metals, they may form a molten shell not denser than is implied by the facts. But this can be regarded as nothing more than a possibility.

No need, however, has arisen for either relinquishing or holding but loosely the associated conclusions respecting the constitution of the photosphere and its envelope. Widely speculative as seemed these suggested corollaries from the Nebular Hypothesis when set forth in 1858, and quite at variance with the beliefs then current, they proved to be not ill-founded. At the close of 1859, there came the discoveries of Kirchhoff, proving the existence of various metallic vapours in the Sun's atmosphere.

by sundry traits, and especially by their occurrence in greater or less abundance where the hypothesis implies greater or less abundance, confirm this testimony. By tracing out the process of planetary condensation, we are led to conclusions respecting the physical states of planets which explain their anomalous specific gravities. Once more, it turns out that what is inferable from the Nebular Hypothesis respecting the temperatures of celestial bodies, is just what observation establishes; and that both the absolute and the relative temperatures of the Sun and planets are thus accounted for. When we contemplate these various evidences in their totality—when we observe that, by the Nebular Hypothesis, the leading phenomena of the Solar System, and the heavens in general, are explicable; and when, on the other hand, we consider that the current cosmogony is not only without a single fact to stand on, but is at variance with all our positive knowledge of Nature, we see that the proof becomes overwhelming.

It remains only to point out that while the genesis of the Solar System, and of countless other systems like it, is thus rendered comprehensible, the ultimate mystery continues as great as ever. The problem of existence is not solved: it is simply removed further back. The Nebular Hypothesis throws no light on the origin of diffused matter; and diffused matter as much needs accounting for as concrete matter. The genesis of an atom is not easier to conceive than the genesis of a planet. Nay, indeed, so far from making the Universe less a mystery than before, it makes it a greater mystery. Creation by manufacture is a much lower thing than creation by evolution. A man can put together a machine; but he cannot make a machine develop itself. That our harmonious universe once existed potentially as formless diffused matter, and has slowly grown into its present organized state, is a far more astonishing fact than would have been its formation after the artificial method vulgarly supposed. Those who hold it legitimate to argue from phenomena to noumena, may rightly contend that the Nebular Hypothesis implies a First Cause as much transcending "the mechanical God of Paley," as this does the fetish of the savage.

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## ADDENDA.

SPECULATIVE as is much of the foregoing essay, it appears undesirable to include in it anything still more speculative. For this reason I have decided to set forth separately some views concerning the genesis of the so-called elements during nebular condensation, and concerning the accompanying physical effects. At the same time it has seemed best to detach from the essay some of the more debatable conclusions originally contained in it; so that its general argument may not be needlessly implicated with them. These new portions, together with the old portions which re-appear more or less modified, I here append in a series of notes.

NOTE I. For the belief that the so-called elements are compound there are both special reasons and general reasons. Among the special may be named the parallelism between allotropy and isomerism; the numerous lines in the spectrum of each element; and the cyclical law of Newlands and Mendeljeff. Of the more general reasons, which, as distinguished from these chemical or chemicophysical ones, may fitly be called cosmical, the following are the chief.

The general law of evolution, if it does not actually involve the conclusion that the so-called elements are compounds, yet affords *a priori* ground for suspecting that they are such. The implication is that, while the matter composing the Solar System has progressed physically from that relatively-homogeneous state which it had as a nebula to that relatively-heterogeneous state presented by Sun, planets, and satellites, it has also progressed chemically, from the relatively-homogeneous state in which it was composed of one or a few types of matter, to that relatively-heterogeneous state in which it is composed of many types of matter very diverse in their properties. This deduction from the law which holds throughout the cosmos as now known to us, would have much weight even were it unsupported by induction; but a survey of chemical phenomena at large discloses several groups of inductive evidences supporting it.

The first is that since the cooling of the Earth reached an advanced stage, the components of its crust have been ever increasing in heterogeneity. When the so-called elements, originally existing in a dissociated state, united into oxides, acids, and other binary compounds, the total number of different substances was immensely augmented, the new substances were more complex than the old, and their properties were more varied. That is, the assemblage became more heterogeneous in its kinds, in the composition of each kind, and in the range of chemical characters. When, at a later period, there arose salts and other compounds of similar degrees of complexity, there was again an increase of heterogeneity, alike in the aggregate and in its members. And when, still later, matters classed as organic became possible, the multiformity was yet further augmented in kindred ways. If, then, chemical evolution, so far as we can trace it, has been from the homogeneous to the heterogeneous, may we not fairly suppose that it has been so from the beginning? If, from late stages in the Earth's history, we run back, and find the lines of chemical evolution continually converging, until they bring us to bodies which we cannot decompose, may we not suspect that, could we run back these lines still further, we should come to still decreasing heterogeneity in the number and nature of the substances, until we reached something like homogeneity?

A parallel argument may be derived from consideration of the affinities and stabilities of chemical compounds. Beginning with the complex nitrogenous bodies out of which living things are formed, and which, in the history of the Earth, are the most modern, at the same time that they are the most heterogeneous, we see that the affinities and stabilities of these are extremely small. Their molecules do not enter bodily into union with those of other substances so as to form more complex compounds still, and their components often fail to hold together under ordinary conditions. A stage lower in degree of composition we come to the vast assemblage of oxy-hydro-carbons, numbers of which show many and decided affinities, and are stable at common temperatures. Passing to the inorganic group, we are shown by the salts &c. strong affinities between their components and unions which are, in many cases, not very easily broken. And then when we come to the oxides, acids, and other binary compounds, we see that in many cases the elements of which they are formed, when brought into the presence of one another under favourable conditions, unite with violence; and that many of their unions cannot be dissolved by heat alone. If, then, as we go back from the most modern and most complex substances to the most ancient and simplest substances, we see, on the average, a great increase in affinity and stability, it results that if the same law holds with the simplest substances known to us, the components of these, if they are compound, may be assumed to have united with affinities far more intense than any we have experience of, and to cling together with tenacities far exceeding the tenacities with which chemistry acquaints us. Hence the existence of a class of substances which are undecomposable and therefore seem simple, appears to be an implication; and the corollary is that these were formed during early stages of terrestrial concentration, under conditions of heat and pressure which we cannot now parallel.

Yet another support for the belief that the so-called elements are compounds, is derived from a comparison of them, considered as an aggregate ascending in their molecular weights, with the aggregate of bodies known to be compound, similarly considered in their ascending molecular weights. Contrast the binary compounds as a class with the quaternary compounds as a class. The molecules constituting oxides (whether alkaline or acid or neutral) chlorides, sulphurets, &c. are relatively small; and, combining with great avidity, form stable compounds. On the other hand, the molecules constituting nitrogenous bodies are relatively vast and are chemically inert; and such combinations as their simpler types enter into, cannot withstand disturbing forces. Now a like difference is seen if we contrast with one another the so-called elements. Those of relatively-low molecular weights—oxygen, hydrogen, potassium, sodium, &c.,—show great readiness to unite among themselves; and, indeed, many of them cannot be prevented from uniting under ordinary conditions. Contrariwise, under ordinary conditions the substances of high molecular weights—the "noble metals"—are indifferent to other substances; and such compounds as they do form under conditions specially adjusted, are easily destroyed. Thus as, among the bodies we know to be compound, increasing molecular weight is associated with the appearance of certain characters, and as, among the bodies we class as simple, increasing molecular weight is associated with the appearance of similar characters, the composite nature of the elements is in another way pointed to.

There has to be added one further class of phenomena, congruous with those above named, which here specially concerns us. Looking generally at chemical unions, we see that the heat evolved usually decreases as the degree of composition, and consequent massiveness, of the molecules, increases. In the first place, we have the fact that during the formation of simple compounds the heat evolved is much greater than that which is evolved during the formation of complex compounds: the elements, when uniting with one another, usually give out much heat; while, when the compounds they form are recompounded, but little heat is given out; and, as shown by the experiments of Prof. Andrews, the heat given out during the union of acids and bases is habitually smaller where the molecular weight of the base is greater. Then, in the second place, we see that among the elements themselves, the unions of those having low molecular weights result in far more heat than do the unions of those having high molecular weights. If we proceed on the supposition that the so-called elements are compounds, and if this law, if not universal, holds of undecomposable substances as of decomposable, then there are two implications. The one is that those compoundings and recompoundings by which the elements were formed, must have been accompanied by degrees of heat exceeding any degrees of heat known to us. The other is that among these compoundings and recompoundings themselves, those by which the small-moleculed elements were formed produced more intense heat than those by which the large-moleculed elements were formed: the elements formed by the final recompoundings being necessarily later in origin, and at the same time less stable, than the earlier-formed ones.

NOTE II. May we from these propositions, and especially from the last, draw any conclusions respecting the evolution of heat during nebular condensation? And do such conclusions affect in any way the conclusions now current?

In the first place, it seems inferable from physico-chemical facts at large, that only through the instrumentality of those combinations which formed the elements, did the concentration of diffused nebulous matter into concrete masses become possible. If we remember that hydrogen and oxygen in their uncombined states oppose, the one an insuperable and the other an almost insuperable, resistance to liquefaction, while when combined the compound assumes the liquid state with facility, we may suspect that in like manner the simpler types of matter out of which the elements were formed, could not have been reduced even to such degrees of density as the known gases show us, without what we may call proto-chemical unions: the implication being that after the heat resulting from each of such proto-chemical unions had escaped, mutual gravitation of the parts was able to produce further condensation of the nebulous mass.

If we thus distinguish between the two sources of heat accompanying nebular condensation—the heat due to proto-chemical combinations and that due to the contraction caused by gravitation (both of them, however, being interpretable as consequent on loss of motion), it may be inferred that they take different shares during the earlier and during the later stages of aggregation. It seems probable that while the diffusion is great and the force of mutual gravitation small, the chief source of heat is combination of units of matter, simpler than any known to us, into such units of matter as those we know; while, conversely, when there has been reached close aggregation, the chief source of heat is gravitation, with consequent pressure and gradual contraction. Supposing this to be so, let us ask what may be inferred. If at the time when the nebulous spheroid from which the Solar System resulted, filled the orbit of

Neptune, it had reached such a degree of density as enabled those units of matter which compose the sodium molecules to enter into combination; and if, in conformity with the analogies above indicated, the heat evolved by this proto-chemical combination was great compared with the heats evolved by the chemical combinations known to us; the implication is that the nebulous spheroid, in the course of its contraction, would have to get rid of a much larger quantity of heat than it would, did it commence at any ordinary temperature and had only to lose the heat consequent on contraction. That is to say, in estimating the past period during which solar emission of heat has been going on at a high rate, much must depend on the initial temperature assumed; and this may have been rendered intense by the proto-chemical changes which took place in early stages.*

Respecting the future duration of the solar heat, there must also be differences between the estimates made according as we do or do not take into account the proto-chemical changes which possibly have still to take place. True as it may be that the quantity of heat to be emitted is measured by the quantity of motion to be lost, and that this must be the same whether the approximation of the molecules is effected by chemical unions, or by mutual gravitation, or by both; yet, evidently, everything must turn on the degree of condensation supposed to be eventually reached; and this must in large measure depend on the natures of the substances eventually formed. Though, by spectrum-analysis, platinum has recently been detected in the solar atmosphere, it seems clear that the metals of low molecular weights greatly predominate; and supposing the foregoing arguments to be valid, it may be inferred, as not improbable, that the compoundings and recompoundings by which the heavy-moleculed elements are produced, not hitherto possible in large measure, will hereafter take place; and that, as a result, the Sun's density will finally become very great in comparison with what it is now. I say "not hitherto possible in large measure", because it is a feasible supposition that they may be formed, and can continue to exist, only in certain outer parts of the Solar mass, where the pressure is sufficiently great while the heat is not too great. And if this be so, the implication is that the interior body of the Sun, higher in temperature than its peripheral layers, may consist wholly of the metals of low atomic weights, and that this may be a part cause of his low specific gravity; and a further implication is that when, in course of time, the internal temperature falls, the heavy-moleculed elements, as they severally become capable of existing in it, may arise: the formation of each having an evolution of heat as its concomitant.* If so, it would seem to follow that the amount of heat to be emitted by the Sun, and the length of the period during which the emission will go on, must be taken as much greater than if the Sun is supposed to be permanently constituted of the elements now predominating in him, and to be capable of only that degree of condensation which such composition permits.

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* Of course there remains the question whether, before the stage here recognized, there had already been produced a high temperature by those collisions of celestial masses which reduced the matter to a nebulous form. As suggested in *First Principles* (§ 136 in the edition of 1862, and § 182 in subsequent editions), there must, after there have been effected all those minor dissolutions which follow evolutions, remain to be effected the dissolutions of the great bodies in and on which the minor evolutions and dissolutions have taken place; and it was argued that such dissolutions will be, at some time or other, effected by those immense transformations of molar motion into molecular motion, consequent on collisions: the argument being based on the statement of Sir John Herschel, that in clusters of stars collisions must inevitably occur. It may, however, be objected that though such a result may be reasonably looked for in closely aggregated assemblages of stars, it is difficult to conceive of its taking place throughout our Sidereal System at large, the members of which, and their intervals, may be roughly figured as pins-heads 50 miles apart. It would seem that something like an eternity must elapse before, by ethereal resistance or other cause, these can be brought into proximity great enough to make collisions probable.

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* The two sentences which, in the text, precede the asterisk, I have introduced while these pages are standing in type: being led to do so by the perusal of some notes kindly lent to me by Prof. Dewar, containing the outline of a lecture he gave at the Royal Institution during the session of 1880. Discussing the conditions under which, if "our so-called elements are compounded of elemental matter", they may have been formed, Prof. Dewar, arguing from the known habitudes of compound substances, concludes that the formation is in each case a function of pressure, temperature, and nature of the environing gases.

NOTE III. Are the internal structures of celestial bodies all the same, or do they differ? And if they differ, can we, from the process of nebular condensation, infer the conditions under which they assume one or other character? In the foregoing essay as originally published, these questions were discussed; and though the conclusions reached cannot be sustained in the form given to them, they foreshadow conclusions which may, perhaps, be sustained. Referring to the conceivable causes of unlike specific gravities in the members of the solar system, it was said that these might be—

> "1. Differences between the kinds of matter or matters composing them. 2. Differences between the quantities of matter; for, other things equal, the mutual gravitation of atoms will make a large mass denser than a small one. 3. Differences between the structures: the masses being either solid or liquid throughout, or having central cavities filled with elastic aëriform substance. Of these three conceivable causes, that commonly assigned is the first, more or less modified by the second."

Written as this was before spectrum-analysis had made its disclosures, no notice could of course be taken of the way in which these conflict with the first of the foregoing suppositions; but after pointing out other objections to it the argument continued thus:—

> "However, spite of these difficulties, the current hypothesis is, that the Sun and planets, inclusive of the Earth, are either solid or liquid, or have solid crusts with liquid nuclei."*

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* At the date of this passage the established teleology made it seem needful to assume that all the planets are habitable, and that even beneath the photosphere of the Sun there exists a dark body which may be the scene of life; but since then, the influence of teleology has so far diminished that this hypothesis can no longer be called the current one.

After saying that the familiarity of this hypothesis must not delude us into uncritical acceptance of it, but that if any other hypothesis is physically possible it may reasonably be entertained, it was argued that by tracing out the process of condensation in a nebulous spheroid, we are led to infer the eventual formation of a molten shell with a nucleus consisting of gaseous matter at high tension. The paragraph which then follows runs thus:—

"But what," it may be asked, "will become of this gaseous nucleus when exposed to the enormous gravitative pressure of a shell some thousands of miles thick? How can aeriform matter withstand such a pressure?" Very readily. It has been proved that, even when the heat generated by compression is allowed to escape, some gases remain uncondensible by any force we can produce. An unsuccessful attempt lately made in Vienna to liquify oxygen, clearly shows this enormous resistance. The steel piston employed was literally shortened by the pressure used; and yet the gas remained unliquified! If, then, the expansive force is thus immense when the heat evolved is dissipated, what must it be when that heat is in great measure detained, as in the case we are considering? Indeed the experiences of M. Cagniard de Latour have shown that gases may, under pressure, acquire the density of liquids while retaining the aeriform state, provided the temperature continues extremely high. In such a case, every addition to the heat is an addition to the repulsive power of the atoms: the increased pressure itself generates an increased ability to resist; and this remains true to whatever extent the compression is carried. Indeed it is a corollary from the persistence of force that if, under increasing pressure, a gas retains all the heat evolved, its resisting force is *absolutely unlimited.* Hence the internal planetary structure we have described is as physically stable a one as that commonly assumed."

Had this paragraph, and the subsequent paragraphs, been written five years later, when Prof. Andrews had published an account of his researches, the propositions they contain, while rendered more specific and at the same time more defensible, would perhaps have been freed from the erroneous implication that the internal structure indicated is an universal one. Let us, while guided by Prof. Andrews' results, consider what would probably be the successive changes in a condensing nebulous spheroid.

Prof. Andrews has shown that for each kind of gaseous matter there is a temperature above which no amount of pressure can cause liquefaction. The remark, made *a priori* in the above extract, "that if, under increasing pressure, a gas retains all the heat evolved, its resisting force is *absolutely unlimited*", harmonizes with the inductively-reached result that if the temperature is not lowered to its "critical point" a gas does not liquify, however great the force applied. At the same time Prof. Andrews' experiments imply that, supposing the temperature to be lowered to the point at which liquefaction becomes possible, then liquefaction will take place where there is first reached the required pressure. What are the corollaries in relation to concentrating nebulous spheroids?

Assume a spheroid of such size as will form one of the inferior planets, and consisting externally of a voluminous, cloudy atmosphere composed of the less condensible elements, and internally of metallic gases: such internal gases being kept by convection-currents at temperatures not very widely differing. And assume that continuous radiation has brought the internal mass of metallic gases down to the critical point of the most condensible. May we not say that there is a size of the spheroid such that the pressure will not be great enough to produce liquefaction at any other place than the centre? or, in other words, that in the process of decreasing temperature and increasing pressure, the centre will be the place at which the combined conditions of pressure and temperature will be first reached? If so, liquefaction, commencing at the centre, will spread thence to the periphery; and, in virtue of the law that solids have higher melting points under pressure than when free, it may be that solidification will similarly, at a later stage, begin at the centre and progress outwards: eventually producing, in that case, a state such as Sir William Thomson alleges exists in the Earth. But now suppose that instead of such a spheroid, we assume one of, say, twenty or thirty times the mass; what will then happen? Notwithstanding convection-currents, the temperature at the centre must always be higher than elsewhere; and in the process of cooling the "critical point" of temperature will sooner be reached in the outer parts. Though the requisite pressure will not exist near the surface, there is evidently, in a large spheroid, a depth below the surface at which the pressure will be great enough, if the temperature is sufficiently low. Hence it is inferable that somewhere between centre and surface in the supposed larger spheroid, there will arise that state described by Prof. Andrews, in which "flickering striæ" of liquid float in gaseous matter of equal density. And it may be inferred that gradually, as the process goes on, these striæ will become more abundant while the gaseous interspaces diminish; until, eventually, the liquid becomes continuous. Thus there will result a molten shell containing a gaseous nucleus equally dense with itself at their surface of contact and more dense at the centre—a molten shell which will slowly thicken by additions to both exterior and interior.

That a solid crust will eventually form on this molten shell may be reasonably concluded. To the demurrer that solidification cannot commence at the surface, because the solids formed would sink, there are two replies. The first is that various metals expand while solidifying, and therefore would float. The second is that since the envelope of the supposed spheroid would consist of the gases and non-metallic elements, compounds of these with the metals and with one another would continually accumulate on the molten shell; and the crust, consisting of oxides, chlorides, sulphurets, and the rest, having much less specific gravity than the molten shell, would be readily supported by it.

Clearly a planet thus constituted would be in an unstable state. Always it would remain liable to a catastrophe resulting from change in its gaseous nucleus. If, under some condition of pressure and temperature eventually reached, the components of this suddenly entered into one of those proto-chemical combinations forming a new element, there might result an explosion capable of shattering the entire planet, and propelling its fragments in all directions with high velocities. If the hypothetical planet between Jupiter and Mars was intermediate in size as in position, it would apparently fulfil the conditions under which such a catastrophe might occur.

NOTE IV. The argument set forth in the foregoing note, is in part designed to introduce a question which seems to require re-consideration—the origin of the minor planets or planetoids. The hypothesis of Olbers, as propounded by him, implied that the disruption of the assumed planet between Mars and Jupiter had taken place at no very remote period in the past; and this implication was shown to be inadmissible by the discovery that there exists no such point of intersection of the orbits of the planetoids as the hypothesis requires. The inquiry whether, in the past, there was any nearer approach to a point of intersection than at present, having resulted in a negative, it is held that the hypothesis must be abandoned. It is, however, admitted that the mutual perturbations of the planetoids themselves would suffice, in the course of some millions of years, to destroy all traces of a place of intersection of their orbits, if it once existed. But if this be admitted why need the hypothesis be abandoned? Given such duration of the Solar System as is currently assumed, there seems no reason why lapse of a few millions of years should present any difficulty. The explosion may as well have taken place ten million years ago as at any more recent period. And whoever grants this must grant that the probability of the hypothesis has to be estimated from other data.

As a preliminary to closer consideration, let us ask what may be inferred from the rate of discovery of the planetoids, and from the sizes of those most recently discovered. In 1878, Prof. Newcomb, arguing that "the preponderance of evidence is on the side of the number and magnitude being limited", says that "the newly discovered ones" "do not seem, on the average, to be materially smaller than those which were discovered ten years ago"; and further that "the new ones will probably be found to grow decidedly rare before another hundred are discovered". Now, inspection of the tables contained in the just-published fourth edition of Chambers' *Descriptive Astronomy* (vol. I) shows that whereas the planetoids discovered in 1868 (the year Prof. Newcomb singles out for comparison) have an average magnitude of 11·56 those discovered last year (1888) have an average magnitude of 12·43. Further, it is observable that though more than ninety have been discovered since Prof. Newcomb wrote, they have by no means become rare: the year 1888 having added ten to the list, and having therefore maintained the average rate of the preceding ten years. If, then, the indications Prof. Newcomb names, had they arisen, would have implied a limitation of the number, these opposite indications imply that the number is unlimited. The reasonable conclusion appears to be that these minor planets are to be counted not by hundreds but by thousands; that more powerful telescopes will go on revealing still smaller ones; and that additions to the list will cease only when the smallness ends in invisibility.

Commencing now to scrutinize the two hypotheses respecting the genesis of these multitudinous bodies, I may first remark concerning that of Laplace, that he might possibly not have propounded it had he known that instead of four such bodies there are hundreds, if not thousands. The supposition that they resulted from the breaking up of a nebulous ring into numerous small portions, instead of its collapse into one mass, might not, in such case, have seemed to him so probable. It would have appeared still less probable had he been aware of all that has since been discovered concerning the wide differences of the orbits in size, their various and often great eccentricities, and their various and often great inclinations. Let us look at these and other incongruous traits of them.

(1.) Between the greatest and least mean distances of the planetoids there is a space of 200 millions of miles; so that the whole of the Earth's orbit might be placed between the limits of the zone occupied, and leave 7 millions of miles on either side: add to which that the widest excursions of the planetoids occupy a zone of 270 millions of miles. Had the rings from which Mercury, Venus, and the Earth were formed been one-sixth of the smaller width or one-ninth of the greater, they would have united: there would have been no nebulous rings at all, but a continuous disk. Nay more, since one of the planetoids trenches upon the orbit of Mars, it follows that the nebulous ring out of which the planetoids were formed must have overlapped that out of which Mars was formed. How do these implications consist with the nebular hypothesis? (2.) The tacit assumption usually made is that the different parts of a nebulous ring have the same angular velocities. Though this assumption may not be strictly true, yet it seems scarcely likely that it is so widely untrue as it would be had the inner part of the ring an angular velocity nearly thrice that of the outer. Yet this is implied. While the period of Thule is 8.8 years, the period of Medusa is 3·1 years. (3.) The eccentricity of Jupiter's orbit is 0·04816, and the eccentricity of Mars' orbit is 0·09311. Estimated by groups of the first found and last found of the planetoids, the average eccentricity of the assemblage is about three times that of Jupiter and more than one and a half times that of Mars; and among the members of the assemblage themselves, some have an eccentricity thirty-five times that of others. How came this nebulous zone, out of which it is supposed the planetoids arose, to have originated eccentricities so divergent from one another as well as from those of the neighbouring planets? (4.) A like question may be asked respecting the inclinations of the orbits. The average inclination of the planetoid-orbits is four times the inclination of Mars' orbit and six times the inclination of Jupiter's orbit; and among the planetoid-orbits themselves the inclinations of some are fifty times those of others. How are all these differences to be accounted for on the hypothesis of genesis from a nebulous ring? (5.) Much greater becomes the difficulty on inquiring how these extremely unlike eccentricities and inclinations came to co-exist before the parts of the nebulous ring separated, and how they survived after the separation. Were all the great eccentricities displayed by the outermost members of the group, and the small by the innermost members, and were the inclinations so distributed that the orbits having much belonged to one part of the group, and those having little to another part of the group; the difficulty of explanation might not be insuperable. But the arrangement is by no means this. The orbits are, to use an expressive word, miscellaneously jumbled. Hence, if we go back to the nebulous ring, there presents itself the question,—How came each planetoid-forming portion of nebulous matter, when it gathered itself together and separated, to have a motion round the Sun differing so much from the motions of its neighbours in eccentricity and inclination? And there presents itself the further question,—How, during the time when it was concentrating into a planetoid, did it manage to jostle its way through all the differently-moving like masses of nebulous matter, and yet to preserve its individuality? Answers to these questions are, it seems to me, not even imaginable.

Turn we now to the alternative hypothesis. During revision of the foregoing essay, in preparation for that edition of the volume containing it which was published in 1883, there occurred the thought that some light on the origin of the planetoids ought to be obtained by study of their distributions and movements. If, as Olbers supposed, they resulted from the bursting of a planet once revolving in the region they occupy, the implications are :—first, that the fragments must be most abundant in the space immediately about the original orbit, and less abundant far away from it; second, that the large fragments must be relatively few, while of smaller fragments the numbers will increase as the sizes decrease; third, that as some among the smaller fragments will be propelled further than any of the larger, the widest deviations in mean distance from the mean distance of the original planet, will be presented by the smallest members of the assemblage; and fourth, that the orbits differing most from the rest in eccentricity and in inclination, will be among those of these smallest members. In the fourth edition of Chambers's *Handbook of Descriptive and Practical Astronomy* (the first volume of which has just been issued) there is a list of the elements (extracted and adapted from the *Berliner Astronomisches Jahrbuch* for 1890) of all the small planets (281 in number) which had been discovered up to the end of 1888. The apparent brightness, as expressed in equivalent star-magnitudes, is the only index we have to the probable comparative sizes of by far the largest number of the planetoids: the exceptions being among those first discovered. Thus much premised, let us take the above points in order. (1) There is a region lying between 2·50 and 2·80 (in terms of the Earth's mean distance from the Sun) where the planetoids are found in maximum abundance. The mean between these extremes, 2·65, is nearly the same as the average of the distances of the four largest and earliest-known of these bodies, which amounts to 2·64. May we not say that the thick clustering about this distance (which is, however, rather less than that assigned for the original planet by Bode's empirical law), in contrast with the wide scattering of the comparatively few whose distances are little more than 2 or exceed 3, is a fact in accordance with the hypothesis in question?* (2) Any table which gives the apparent magnitudes of the planetoids, shows at once how much the number of the smaller members of the assemblage exceeds that of those which are comparatively large; and every succeeding year has emphasized this contrast more strongly. Only one of them (Vesta) exceeds in brightness the seventh star-magnitude, while one other (Ceres) is between the seventh and eighth, and a third (Pallas) is above the eighth; but between the eighth and ninth there are six; between the ninth and tenth, twenty; between the tenth and eleventh, fifty-five; below the eleventh a much larger number is known, and the number existing is probably far greater,—a conclusion we cannot doubt when the difficulty of finding the very faint members of the family, visible only in the largest telescopes, is considered. (3) Kindred evidence is furnished if we broadly contrast their mean distances. Out of the 13 largest planetoids whose apparent brightnesses exceed that of a star of the 9·5 magnitude, there is not one having a mean distance that exceeds 3. Of those having magnitudes at least 9·5 and smaller than 10, there are 15; and of these one only has a mean distance greater than 3. Of those between 10 and 10·5 there are 17; and of these also there is one exceeding 3 in mean distance. In the next group there are 37, and of these 5 have this great mean distance. The next group, 48, contains 12 such; the next, 47, contains 13 such. Of those of the twelfth magnitude and fainter, 72 planetoids have been discovered, and of those of them of which the orbits have been computed, no fewer than 23 have a mean distance exceeding 3 in terms of the Earth's. It is evident from this how comparatively erratic are the fainter members of the extensive family with which we are dealing. (4) To illustrate the next point, it may be noted that among the planetoids whose sizes have been approximately measured, the orbits of the two largest, Vesta and Ceres, have eccentricities falling between ·05 and ·10, whilst the orbits of the two smallest, Menippe and Eva, have eccentricities falling between ·20 and ·25, and between ·30 and ·35. And then among those more recently discovered, having diameters so small that measurement of them has not been practicable, come the extremely erratic ones,—Hilda and Thule, which have mean distances of 3·97 and 4·25 respectively; Æthra, having an orbit so eccentric that it cuts the orbit of Mars; and Medusa, which has the smallest mean distance from the Sun of any. (5) If the average eccentricities of the orbits of the planetoids grouped according to their decreasing sizes are compared, no very definite results are disclosed, excepting this, that the eight Polyhymnia, Atalanta, Eurydice, Æthra, Eva, Andromache, Istria, and Eudora, which have the greatest eccentricities (falling between ·30 and ·38), are all among those of smallest star-magnitudes. Nor when we consider the inclinations of the orbits do we meet with obvious verifications; since the proportion of highly-inclined orbits among the smaller planetoids does not appear to be greater than among the others. But consideration shows that there are two ways in which these last comparisons are vitiated. One is that the inclinations are measured from the plane of the ecliptic, instead of being measured from the plane of the orbit of the hypothetical planet. The other, and more important one, is that the search for planetoids has naturally been carried on in that comparatively narrow zone within which most of their orbits fall; and that, consequently, those having the most highly-inclined orbits are the least likely to have been detected, especially if they are at the same time among the smallest. Moreover, considering the general relation between the inclination of planetoid orbits and their eccentricities, it is probable that among the orbits of these undetected planetoids are many of the most eccentric. But while recognizing the incompleteness of the evidence, it seems to me that it goes far to justify the hypothesis of Olbers, and is quite incongruous with that of Laplace. And as having the same meanings let me not omit the remarkable fact concerning the planetoids discovered by D'Arrest, that "if their orbits are figured under the form of material rings, these rings will be found so entangled, that it would be possible, by means of one among them taken at hazard, to lift up all the rest,"—a fact incongruous with Laplace's hypothesis, which implies an approximate concentricity, but quite congruous with the hypothesis of an exploded planet.

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* It may here be mentioned (though the principal significance of this comes under the next head) that the average mean distance of the later-discovered planetoids is somewhat greater than that of these earlier-discovered; amounting to 2·61 for Nos. 1 to 35 and 2·80 for Nos. 211 to 245. For this observation I am indebted to Mr. Lynn; whose attention was drawn to it while revising for me the statements contained in this paragraph, so as to include discoveries made since the paragraph was written.

Next to be considered come phenomena, the bearings of which on the question before us are scarcely considered—I mean those presented by meteors and shooting stars. The natures and distributions of these harmonize with the hypothesis of an exploded planet, and I think with no other hypothesis. The theory of volcanic origin, joined with the remark that the Sun emits jets which might propel them with adequate velocities, seems quite untenable. Such meteoric bodies as have descended to us, forbid absolutely the supposition of solar origin. Nor can they rationally be ascribed to planetary volcanoes. Even were their mineral characters appropriate, which many of them are not (for volcanoes do not eject iron), no planetary volcanoes could propel them with anything like the implied velocity—could no more withstand the tremendous force to be assumed, than could a card-board gun the force behind a rifle bullet. But that their mineral characters, various as they are, harmonize with the supposition that they were derived from the crust of a planet is manifest; and that the bursting of a planet might give to them, and to shooting stars, the needful velocities, is a reasonable conclusion. Along with those larger fragments of the crust constituting the known planetoids, varying from some 200 miles in diameter to little over a dozen, there would be sent out still more multitudinous portions of the crust, decreasing in size as they increased in number. And while there would thus result such masses as occasionally fall through the Earth's atmosphere to its surface, there would, in an accompanying process, be an adequate cause for the myriads of far smaller masses which, as shooting stars, are dissipated in passing through the Earth's atmosphere. Let us figure to ourselves, as well as we may, the process of explosion.

Assume that the diameter of the missing planet was 20,000 miles; that its solid crust was a thousand miles thick; that under this came a shell of molten metallic matter which was another thousand miles thick; and that the space, 16,000 miles in diameter, within this, was occupied by the equally dense mass of gases above the "critical point", which, entering into a proto-chemical combination, caused the destroying explosion. The primary fissures in the crust must have been far apart—probably averaging distances between them as great as the thickness of the crust. Supposing them approximately equidistant, there would, in the equatorial periphery, be between 60 and 70 fissures. By the time the primary fragments thus separated had been heaved a mile outwards, the fissures formed would severally have, at the surface, a width of 170 odd yards. Of course these great masses, as soon as they moved, would themselves begin to fall in pieces; especially at their bounding surfaces. But passing over the resulting complications, we see that when the masses had been propelled 10 miles outwards, the fissures between them would be each a mile wide. Notwithstanding the enormous forces at work, an appreciable interval would elapse before these vast portions of the crust could be put in motion with any considerable velocities. Perhaps the estimate will be under the mark if we assume that it took 10 seconds to propel them through the first mile, and that, by implication, at the end of 20 seconds they had travelled 4 miles, and at the end of 30 seconds 9 miles. Supposing this granted, let us ask what would be taking place in each intervening fissure a thousand miles deep, which, in the space of half a minute, had opened out to nearly a mile wide, and in the subsequent half minute to a chasm approaching 3 miles in width. There would first be propelled through it enormous jets of the molten metals composing the internal liquid shell; and these would part into relatively small masses as they were shot into space. Presently, as the chasm opened to some miles in width, the molten metals would begin to be followed by the equally dense gaseous matter behind, and the two would rush out together. Soon the gases, predominating, would carry with them the portions of the liquid shell continually collapsing; until the blast became one filled with millions of small masses, billions of smaller masses, and trillions of drops. These would be driven into space in a stream, the emission of which would continue for many seconds or even several minutes. Remembering the rate of motion of the jets emitted from the solar surface, and supposing that the blasts produced by this explosion reached only one-tenth of that rate, these myriads of small masses and drops would be propelled with planetary velocities, and in approximately the same direction. I say approximately, because they would be made to deviate somewhat by the friction and irregularities of the chasm passed through, and also by the rotation of the planet. Observe, however, that though they would all have immense velocities, their velocities would not be equal. During its earlier stages the blast would be considerably retarded by the resistance which the sides of its channel offered. When this became relatively small the velocity of the blast would reach its maximum; from which it would decline when the space for emission became very wide, and the pressure behind consequently less. Hence these almost infinitely numerous particles of planet-spray, as we might call it, as well as those formed by the condensation of the metallic vapours accompanying them, would forthwith begin to part-company: some going rapidly in advance, and others falling behind; until the stream of them, perpetually elongating, formed an orbit round the Sun, or rather an assemblage of innumerable orbits, separating widely at aphelion and perihelion, but approximating midway, where they might fall within a space of, say, some two millions of miles, as do the orbits of the November meteors. At a later stage of the explosion, when the large masses, having moved far outwards, had also fallen to pieces of every size, from that of Vesta to that of an aerolite, and when the channels just described had ceased to exist, the contents of the planet would disperse themselves with lower velocities and without any unity of direction. Hence we see causes alike for the streams of shooting stars, for the solitary shooting stars visible to the naked eye, and for the telescopic shooting stars a score times more numerous.

Further significant evidence is furnished by the comets of short periods. Of the thirteen constituting this group, twelve have orbits falling between those of Mars and Jupiter: one only having its aphelion beyond the orbit of Jupiter. That is to say, nearly all of them frequent the same region as the planetoids. By implication, they are similarly associated in respect of their periods. The periods of the planetoids range from 3.1 to 8.8 years; and all these twelve comets have periods falling between these extremes: the least being 3.29 and the greatest 8.86. Once more this family of comets, like the planetoids in the zone they occupy and like them in their periods, are like them also in the respect that, as Mr. Lynn has pointed out, their motions are all direct. How happens this close kinship—how happens there to be this family of comets so much like the planetoids and so much like one another, but so unlike comets at large? The obvious suggestion is that they are among the products of the explosion which originated the planetoids, the aerolites, and the streams of meteors; and consideration of the probable circumstances shows us that such products might be expected. If the hypothetical planet was like its neighbour Jupiter in having an atmosphere, or like its neighbour Mars in having water on its surface, or like both in these respects; then these superficial masses of liquid, of vapour, and of gas, blown into space along with the solid matters, would yield the materials for comets. There would result, too, comets unlike one another in constitution. If a fissure opened beneath one of the seas, the molten metals and metallic gases rushing through it as above described, would decompose part of the water carried with them; and the oxygen and hydrogen liberated would be mingled with undecomposed vapour. In other cases, portions of the atmosphere might be propelled, probably with portions of vapour; and in yet other cases masses of water alone. Severally subject to great heat at perihelion, these would behave more or less differently. Once more, it would ordinarily happen that detached swarms of meteors projected as implied, would carry with them masses of vapours and gases; whence would result the cometic constitution now insisted on. And sometimes there would be like accompaniments to meteoric streams.

See, then, the contrast between the two hypotheses. That of Laplace, looking probable while there were only four planetoids, but decreasing in apparent likelihood as the planetoids increase in number, until, as they pass through the hundreds on their way to the thousands, it becomes obviously improbable, is, at the same time, otherwise objectionable. It pre-supposes a nebulous ring of a width so enormous that it would have overlapped the ring of Mars. This ring would have had differences between the angular velocities of its parts quite inconsistent with the Nebular

Hypothesis. The average eccentricities of the orbits of its parts must have differed greatly from those of adjacent orbits; and the average inclinations of the orbits of its parts must similarly have differed greatly from those of adjacent orbits. Once more, the orbits of its parts, confusedly interspersed, must have had varieties of eccentricity and inclination unaccountable in portions of the same nebulous ring; and, during concentration into planetoids, each must have had to maintain its course while struggling through the assemblage of other small nebulous masses, severally moving in ways unlike its own. On the other hand, the hypothesis of an exploded planet is supported by every increase in the number of planetoids discovered; by the greater numbers of the smaller sizes; by the thicker clustering near the inferred place of the missing planet; by the occurrence of the greatest mean distances among the smallest members of the assemblage; by the occurrence of the greatest eccentricities in the orbits of these smallest members; and by the entanglement of all the orbits. Further support for the hypothesis is yielded by aerolites, so various in their kinds, but all suggestive of a planet's crust; by the streams of shooting stars having their radiant points variously placed in the heavens; and also by the solitary shooting stars visible to the naked eye, and the more numerous ones visible through telescopes. Once more, it harmonizes with the discovery of a family of comets, twelve out of thirteen of which have mean distances falling within the zone of the planetoids, have similarly associated periods, have all the same direct motions, and are connected with swarms of meteors and with meteoric streams. May we not, indeed, say, that if there once existed a planet between Mars and Jupiter which burst, the explosion must have produced just such clusters of bodies and classes of phenomena as we actually find?

And what is the objection? Merely that if such an explosion occurred it must have occurred many millions of years ago—an objection which is in fact no objection; for the supposition that the explosion occurred many millions of years ago is just as reasonable as the supposition that it occurred recently.

It is, indeed, further objected that some of the resulting fragments ought to have retrograde motions. It turns out on calculation, however, that this is not the case. Assuming as true the velocity which Lagrange estimated would have sufficed to give the four chief planetoids the positions they occupy, it results that such a velocity, given to the fragments which were propelled backwards by the explosion, would not have given them retrograde motions, but would simply have reduced their direct motions from something over 11 miles per second to about 6 miles per second. It is, however, manifest that this reduction of velocity would have necessitated the formation of highly-elliptic orbits—more elliptic than any of those at present known. This seems to me the most serious difficulty which has presented itself. Still, considering that there remain probably an immense number of planetoids to be discovered, it is quite possible that among these there may be some having orbits answering to the requirement.

NOTE V. Shortly before I commenced the revision of the foregoing essay, friends on two occasions named to me some remarkable photographs of nebulæ recently obtained by Mr. Isaac Roberts, and exhibited at the Royal Astronomical Society: saying that they presented appearances such as might have been sketched by Laplace in illustration of his hypothesis. Mr. Roberts has been kind enough to send me copies of the photographs in question and sundry others illustrative of stellar evolution. Those representing the Great Nebulæ in Andromeda and Canum Venaticorum as well as 81 Messier are at once impressive and instructive —illustrating as they do the genesis of nebulous rings round a central mass.

I may remark, however, that they seem to suggest the need for some modification of the current conception; since they make it tolerably clear that the process is a much less uniform one than is supposed. The usual idea is that a vast rotating nebulous spheroid arises before there are produced any of the planet-forming rings. But both of these photographs apparently imply that, in some cases at any rate, the portions of nebulous matter composing the rings take shape before they reach the central mass. It looks as though these partially-formed annuli must be prevented by their acquired motions from approaching even very near to the still-irregular body they surround.

Be this as it may, however, and be the dimensions of the incipient systems what they may (and it would seem to be a necessary implication that they are vastly larger than our Solar System), the process remains essentially the same. Practically demonstrated as this process now is, we may say that the doctrine of nebular genesis passes from the region of hypothesis into the region of established truth.

# THE CONSTITUTION OF THE SUN.

[*First published in* The Reader *for February* 25, 1865. *I reproduce this essay chiefly to give a place to the speculation concerning the solar spots which forms the latter portion of it.*]

THE hypothesis of M. Faye, described in your numbers for January 28 and February 4, respectively, is to a considerable extent coincident with one which I ventured to suggest in an article on "Recent Astronomy and the Nebular Hypothesis," published in the *Westminster Review* for July, 1858. In considering the possible causes of the immense differences of specific gravity among the planets, I was led to question the validity of the tacit assumption that each planet consists of solid or liquid matter from centre to surface. It seemed to me that any other internal structure which was mechanically stable, might be assumed with equal legitimacy. And the hypothesis of a solid or liquid shell, having its cavity filled with gaseous matter at high pressure and temperature [and of great density], was one which seemed worth considering.

Hence arose the inquiry—What structure will result from the process of nebular condensation? [Here followed a long speculation respecting the processes going on in a concentrating nebulous spheroid; the general outcome of which is implied in Note III of the foregoing essay. I do not reproduce it because, not having the guidance of Prof. Andrew's researches, I had concluded that the formation of molten shell would occur universally, instead of occasionally, as is now argued in the note named. The essay then proceeded thus :—]

The process of condensation being in its essentials the same for all concentrating nebular spheroids, planetary or solar, it was argued that the Sun is still passing through that incandescent stage which all the planets have long ago passed through : his later aggregation, joined with the immensely greater ratio of his mass to his surface, involving comparative lateness of cooling. Supposing the sun to have reached the state of a molten shell, inclosing a gaseous nucleus, it was concluded that this molten shell, ever radiating its heat, but ever acquiring fresh heat by further integration of the Sun's mass, must be constantly kept up to that temperature at which its substance evaporates.

[Here followed part of the paragraph quoted in the preceding essay on p. 155; and there succeeded, in subsequent editions, a paragraph aiming to show that the inferred structure of the Sun's interior was congruous with the low specific gravity of the Sun—a conclusion which, as indicated on p. 156, implies some very problematical assumptions respecting the natures of the unknown elements of the Sun. There then came this passage :—]

The conception of the Sun's constitution thus set forth, is like that of M. Faye in so far as the successive changes, the resulting structures, and the ultimate state, are concerned; but unlike it in so far as the Sun is supposed to have reached a later stage of concentration. As I gather from your abstract of M. Faye's paper [this referred to an article in *The Reader*], he considers the Sun to be at present a gaseous spheroid, having an envelope of metallic matters precipitated in the shape of luminous clouds, the local dispersions of which, caused by currents from within, appear to us as spots; and he looks forward to the future formation of a liquid film as an event that will soon be followed by extinction. Whereas the above hypothesis is that the liquid film already exists beneath the visible photosphere, and that extinction cannot result until, in the course of further aggregation, the gaseous nucleus has become so much reduced, and the shell so much thickened, that the escape of the heat generated is greatly retarded. . . . M. Faye's hypothesis appears to be espoused by him, partly because it affords an explanation of the spots, which are considered as openings in the photosphere, exposing the comparatively non-luminous gases filling the interior. But if these interior gases are non-luminous from the absence of precipitated matter, must they not for the same reason be transparent? And if transparent, will not the light from the remote side of the photosphere seen through them, be nearly as bright as that of the side next to us? By as much as the intensely-heated gases of the interior are disabled by the dissociation of their molecules from giving off luminiferous undulations, by so much must they be disabled from absorbing the light transmitted through them. And if their great light-transmitting power is exactly complementary to their small light-emitting power, there seems no reason why the interior of the Sun, disclosed to us by openings in the photosphere, should not appear as bright as its exterior.

Take, on the other hand, the supposition that a more advanced state of concentration has been reached. A shell of molten metallic matter enclosing a gaseous nucleus still higher in temperature than itself, will be continually kept at the highest temperature consistent with its state of liquid aggregation. Unless we assume that simple radiation suffices to give off all the heat generated by progressing integration, we must conclude that the mass will be raised to that temperature at which part of its heat is absorbed in vaporizing its superficial parts. The atmosphere of metallic gases hence resulting, cannot continue to accumulate without reaching a height above the Sun's surface, at which the cooling due to radiation and rarefaction will cause condensation into cloud—cannot, indeed, cease accumulating until the precipitation from the upper limit of the atmosphere balances the evaporation from its lower limit. This upper limit of the atmosphere of metallic gases, whence precipitation is perpetually taking place, will form the visible photosphere—partly giving off light of its own, partly letting through the more brilliant light of the incandescent mass below. This conclusion harmonizes with the appearances. Sir John Herschel, advocating though he does an antagonist hypothesis, gives a description of the Sun's surface which agrees completely with the processes here supposed. He says :—

> "There is nothing which represents so faithfully this appearance as the slow subsidence of some flocculent chemical precipitates in a transparent fluid, when viewed perpendicularly from above: so faithfully, indeed, that it is hardly possible not to be impressed with the idea of a luminous medium intermixed, but not confounded, with a transparent and non-luminous atmosphere, either floating as clouds in our air, or pervading it in vast sheets and columns like flame, or the streamers of our northern lights".—*Treatise on Astronomy*, p. 208.

If the constitution of the Sun be that which is above inferred, it does not seem difficult to conceive still more specifically the production of these appearances. Everywhere throughout the atmosphere of metallic vapours which clothes the solar surface, there must be ascending and descending currents. The magnitude of these currents must obviously depend on the depth of this atmosphere. If it is shallow, the currents must be small; but if many thousands of miles deep, the currents may be wide enough to render visible to us the places at which they severally impinge on the limit of the atmosphere, and the places whence the descending currents commence. The top of an ascending current will be a space over which the thickness of condensed cloud is the least, and through which the greatest amount of light from beneath penetrates. The clouds perpetually formed at the top of such a current, will be perpetually thrust aside by the uncondensed gases from below them; and, growing while they are thrust aside, will collect in the spaces between the ascending currents, where there will result the greatest degree of opacity. Hence the mottled appearance—hence the "pores," or dark interspaces, separating the light-giving spots.*

Of the more special appearances which the photosphere presents, let us take first the faculæ. These are ascribed to waves in the photosphere; and the way in which such waves might produce an excess of light has been variously explained in conformity with various hypotheses. What would result from them in a photosphere constituted and conditioned as above supposed? Traversing a canopy of cloud, here thicker and there thinner, a wave would cause a disturbance very unlikely to leave the thin and thick parts without any change in their average permeability to light. There would probably be, at some parts of the wave, extensions in the areas of the light-transmitting clouds, resulting in the passage of more rays from below. Another phenomenon, less common but more striking, appears also to be in harmony with the hypothesis. I refer to those bright spots, of a brilliancy greater than that of the photosphere, which are sometimes observed. In the course of a physical process so vast and so active as that here supposed to be going on in the Sun, we may expect that concurrent causes will occasionally produce ascending currents much hotter than usual, or more voluminous, or both. One of these, on reaching the stratum of luminous and illuminated cloud forming the photosphere, will burst through it, dispersing and dissolving it, and ascending to a greater height before it begins itself to condense: meanwhile allowing to be seen, through its transparent mass, the incandescent molten shell of the sun's body.

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* If the "rice-grain" appearance is thus produced by the tops of the ascending currents (and M. Faye accepts this interpretation), then I think it excludes M. Faye's hypothesis that the Sun is gaseous throughout. The comparative smallness of the light-giving spots and their comparative uniformity of size, show us that they have ascended through a stratum of but moderate depth (say 10,000 miles), and that this stratum has a *definite* lower limit. This favours the hypothesis of a molten shell.

[The foregoing passages, to most of which I do not commit myself as more than possibilities, I republish chiefly as introductory to the following speculation, which, since it was propounded in 1865, has met with some acceptance.]

"But what of the spots commonly so called?" it will be asked. In the essay on the Nebular hypothesis, above quoted from, it was suggested that refraction of the light passing through the depressed centres of cyclones in this atmosphere of metallic gases, might possibly be the cause; but this, though defensible as a "true cause," appeared on further consideration to be an inadequate cause. Keeping the question in mind, however, and still taking as a postulate the conclusion of Sir John Herschel, that the spots are in some way produced by cyclones, I was led, in the course of the year following the publication of the essay, to an hypothesis which seemed more satisfactory. This, which I named at the time to Prof. Tyndall, had a point in common with the one afterward published by Prof. Kirchhoff, in so far as it supposed cloud to be the cause of darkness; but differed in so far as it assigned the cause of such cloud. More pressing matters prevented me from developing the idea for some time; and, afterwards, I was deterred from including it in the revised edition of the essay, by its inconsistency with the "willow-leaf" doctrine, at that time dominant. The reasoning was as follows:—The central region of a cyclone must be a region of rarefaction, and, consequently, a region of refrigeration. In an atmosphere of metallic gases rising from a molten surface, and presently reaching a limit at which condensation takes place, the molecular state, especially toward its upper part, must be such that a moderate diminution of density, and fall of temperature, will cause precipitation. That is to say, the rarefied interior of a solar cyclone will be filled with cloud: condensation, instead of taking place only at the level of the photosphere, will here extend to a great depth below it, and over a wide area. What will be the characters of a cloud thus occupying the interior of a cyclone? It will have a rotatory motion; and this it has been seen to have. Being funnel-shaped, as analogy warrants us in assuming, its central parts will be much deeper than its peripheral parts, and therefore more opaque. This, too, corresponds with observation. Mr. Dawes has discovered that in the middle of the spot there is a blacker spot: just where there would exist a funnel-shaped prolongation of the cyclonic cloud down toward the Sun's body, the darkness is greater than elsewhere. Moreover, there is furnished an adequate reason for the depression which one of these dark spaces exhibits. In a whirlwind, as in a whirlpool, the vortex will be below the general level, and all around, the surface of the medium will descend toward it. Hence a spot seen obliquely, as when carried toward the Sun's limb, will have its umbra more and more hidden, while its penumbra still remains visible. Nor are we without some interpretation of the penumbra. If, as is implied by what has been said, the so-called "willow-leaves," or "rice-grains," are the tops of the currents ascending from the Sun's body, what changes of appearance are they likely to undergo in the neighbourhood of a cyclone? For some distance round a cyclone there will be a drawing in of the superficial gases toward the vortex. All the luminous spaces of more transparent cloud forming the adjacent photosphere, will be changed in shape by these centripetal currents. They will be greatly elongated; and there will so be produced that "thatch"-like aspect which the penumbra presents.

[The explanation of the solar spots above suggested, which was originally propounded in opposition to that of M. Faye, was eventually adopted by him in place of his own. In the *Comptes Rendus* for 1867, Vol. LXIV., p. 404, he refers to the article in the *Reader*, partly reproduced above, and speaks of me as having been replied to in a previous note. Again in the *Comptes Rendus* for 1872, Vol. LXXV., p. 1664, he recognizes the inadequacy of his hypothesis, saying:—"Il est certain que l'objection de M. Spencer, reproduit et développée par M. Kirchoff, est fondée jusqu'à un certain point; l'intérieur des taches, si ce sont des lacunes dans la photosphère, doit être froid relativement. . . . Il est donc impossible qu'elles proviennent d'éruptions ascendantes." He then proceeds to set forth the hypothesis that the spots are caused by the precipitation of vapour in the interiors of cyclones. But though, as above shown, he refers to the objection made in the foregoing essay to his original hypothesis, and recognizes its cogency, he does not say that the hypothesis which he thereupon substitutes is also to be found in the foregoing essay. Nor does he intimate this in the elaborate paper on the subject read before the French Association for the Advancement of Science, and published in the *Revue Scientifique* for the 24th March 1883. The result is that the hypothesis is now currently ascribed to him.*

About four months before I had to revise this essay on "The Constitution of the Sun," while staying near Pewsey, in Wiltshire, I was fortunate enough to witness a phenomenon which furnished, by analogy, a verification of the above hypothesis, and served more especially to elucidate one of the traits of solar spots, otherwise difficult to understand. It was at the close of August, when there had been a spell of very hot weather. A slight current of air from the West, moving along the line of the valley, had persisted through the day, which, up to 5 o'clock, had been cloudless, and, with the exception now to be named, remained cloudless. The exception was furnished by a strange-looking cloud almost directly overhead. Its central part was comparatively dense and structureless. Its peripheral part, or to speak strictly, the two-thirds of it which were nearest and most clearly visible, consisted of *converging streaks* of comparatively thin cloud. Possibly the third part on the remoter side was similarly constituted; but this I could not see. It did not occur to me at the time to think about its cause, though, had the question been raised, I should doubtless have concluded that as the sky still remained cloudless everywhere else, this precipitated mass of vapour must have resulted from a local eddy. In the space of perhaps half-an-hour, the gentle breeze had carried this cloud some miles to the East; and now its nature became obvious. That central part which, seen from underneath, seemed simply a dense, confused part, apparently no nearer than the rest, now, seen sideways, was obviously much lower than the rest and rudely funnel-shaped—nipple-shaped one might say; while the wide thin portion of cloud above it was disk-shaped: the converging streaks of cloud being now, in perspective, merged together. It thus became manifest that the cloud was produced by a feeble whirlwind, perhaps a quarter to half-a-mile in diameter. Further, the appearances made it clear that this feeble whirlwind was limited to the lower stratum of air: the stratum of air above it was not implicated in the cyclonic action. And then, lastly, there was the striking fact that the upper stratum, though not involved in the whirl, was, by its proximity to a region of diminished pressure, slightly rarified; and that its precipitated vapour was, by the draught set up towards the vortex below, drawn into converging streaks. Here, then, was an action analogous to that which, as above suggested, happens around a sun-spot, where the masses of illuminated vapour constituting the photosphere are drawn towards the vortex of the cyclone, and simultaneously elongated into striæ: so forming the penumbra. At the same time there was furnished an answer to the chief objection to the cyclonic theory of solar spots. For if, as here seen, a cyclone in a lower stratum may fail to communicate a vortical motion to the stratum above it, we may comprehend how, in a solar cyclone, the photosphere commonly fails to give any indication of the revolving currents below, and is only occasionally so entangled in these currents as itself to display a vortical motion.

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* I should add that while M. Faye ascribes solar spots to clouds formed within cyclones, we differ concerning the nature of the cloud. I have argued that it is formed by rarefaction, and consequent refrigeration, of the metallic gases constituting the stratum in which the cyclone exists. He argues that it is formed within the mass of cooled hydrogen drawn from the chromosphere into the vortex of the cyclone. Speaking of the cyclones he says:—"Dans leur embouchure évasée ils entraîneront l'hydrogène froid de la chromosphère, produisant partout sur leur trajet vertical un abaissement notable de température et une obscurité relative, due à l'opacité de l'hydrogène froid englouti." (*Revue Scientifique*, 24 March 1883.) Considering the intense cold required to reduce hydrogen to the "critical point," it is a strong supposition that the motion given to it by fluid friction on entering the vortex of the cyclone, can produce a rotation, rarefaction, and cooling, great enough to produce precipitation in a region so intensely heated.

Let me add that apart from the elucidations furnished by the phenomenon above described, the probabilities are greatly in favour of the cyclonic origin of the solar spots. That some of them exhibit clear marks of vortical motion is undeniable; and if this is so, the question arises—What is the degree of likelihood that there are two causes for spots? Considering that they have so many characters in common, it is extremely improbable that their common characters are in some cases the concomitants of vortical motion and in other cases the concomitants of a different kind of action. Recognizing this great improbability, even in the absence of a reconciliation between the apparently conflicting traits, it is, I think, clear that when, in the way above shown, we are enabled to understand how it happens that the vortical motion, not ordinarily implicating the photosphere, may consequently be in most cases unapparent, the reasons for accepting the cyclonic theory become almost conclusive.]

# ILLOGICAL GEOLOGY.

[*First published in* The Universal Review *for July*, 1859.]

THAT proclivity to generalization which is possessed in greater or less degree by all minds, and without which, indeed, intelligence cannot exist, has unavoidable inconveniences. Through it alone can truth be reached; and yet it almost inevitably betrays into error. But for the tendency to predicate of every other case, that which has been found in the observed cases, there could be no rational thinking; and yet by this indispensable tendency, men are perpetually led to found, on limited experience, propositions which they wrongly assume to be universal or absolute. In one sense, however, this can scarcely be regarded as an evil; for without premature generalizations the true generalization would never be arrived at. If we waited till all the facts were accumulated before trying to formulate them, the vast unorganized mass would be unmanageable. Only by provisional grouping can they be brought into such order as to be dealt with; and this provisional grouping is but another name for premature generalization. How uniformly men follow this course, and how needful the errors are as steps to truth, is well illustrated in the history of Astronomy. The heavenly bodies move round the Earth in circles, said the earliest observers: led partly by the appearances, and partly by their experiences of central motions in terrestrial objects, with which, as all circular, they classed the celestial motions from lack of any alternative conception. Without this provisional belief, wrong as it was, there could not have been that comparison of positions which showed that the motions are not representable by circles; and which led to the hypothesis of epicycles and eccentrics. Only by the aid of this hypothesis, equally untrue, but capable of accounting more nearly for the appearances, and so of inducing more accurate observations —only thus did it become possible for Copernicus to show that the heliocentric theory is more feasible than the geocentric theory; or for Kepler to show that the planets move round the sun in ellipses. Yet again, without the aid of Kepler's more advanced theory of the Solar system, Newton could not have established that general law from which it follows, that the motion of a heavenly body is not necessarily in an ellipse, but may be in any conic section. And lastly, it was only after the law of gravitation had been verified, that it became possible to determine the actual courses of planets, satellites, and comets; and to prove that, in consequence of perturbations, their orbits always deviate, more or less, from regular curves. In these successive theories we may trace both the tendency men have to leap from scanty data to wide generalizations, that are either untrue or but partially true; and the necessity there is for such transitional generalizations as steps to the final one.

In the progress of geological speculation, the same laws of thought are displayed. We have dogmas that were more than half false, passing current for a time as universal truths. We have evidence collected in proof of these dogmas; by and by a colligation of facts in antagonism with them; and eventually a consequent modification. In conformity with this improved hypothesis, we have a better classification of facts; a greater power of arranging and interpreting the new facts now rapidly gathered together;

and further resulting corrections of hypothesis. Being, as we are at present, in the midst of this process, it is not possible to give an adequate account of the development of geological science as thus regarded: the earlier stages are alone known to us. Not only, however, is it interesting to observe how the more advanced views now received respecting the Earth's history, have been evolved out of the crude views which preceded them; but we shall find it extremely instructive to observe this. We shall see how greatly the old ideas still sway both the general mind and the minds of geologists themselves. We shall see how the kind of evidence that has in part abolished these old ideas, is still daily accumulating, and threatens to make other like revolutions. In brief, we shall see whereabouts we are in the elaboration of a true theory of the Earth; and, seeing our whereabouts, shall be the better able to judge, among various conflicting opinions, which best conform to the ascertained direction of geological discovery.

It is needless here to enumerate the many speculations which were in earlier ages propounded by acute men—speculations some of which contained portions of truth. Falling in unfit times, these speculations did not germinate; and hence do not concern us. We have nothing to do with ideas, however good, out of which no science grew; but only with those which gave origin to the existing system of Geology. We therefore begin with Werner.

Taking for data the appearances of the Earth's crust in a narrow district of Germany; observing the constant order of superposition of strata, and their respective physical characters; Werner drew the inference that strata of like characters succeeded each other in like order over the entire surface of the Earth. And seeing, from the laminated structure of many formations and the organic remains contained in others, that they were sedimentary; he further inferred that these universal strata had been in succession precipitated from a chaotic menstruum which once covered our planet. Thus, on a very incomplete acquaintance with a thousandth part of the Earth's crust, he based a sweeping generalization applying to the whole of it. This Neptunist hypothesis, mark, borne out though it seemed to be by the most conspicuous surrounding facts, was quite untenable if analyzed. That a universal chaotic menstruum should deposit a series of numerous sharply-defined strata, differing from one another in composition, is incomprehensible. That the strata so deposited should contain the remains of plants and animals, which could not have lived under the supposed conditions, is still more incomprehensible. Physically absurd, however, as was this hypothesis, it recognized, though under a distorted form, one of the great agencies of geological change—the action of water. It served also to express the fact, that the formations of the Earth's crust stand in some kind of order. Further, it did a little towards supplying a nomenclature, without which much progress was impossible. Lastly, it furnished a standard with which successions of strata in various regions could be compared, the differences noted, and the actual sections tabulated. It was the first provisional generalization; and was useful, if not indispensable, as a step to truer ones.

Following this rude conception, which ascribed geological phenomena to one agency, acting during one primeval epoch, there came a greatly-improved conception, which ascribed them to two agencies, acting alternately during successive epochs. Hutton, perceiving that sedimentary deposits were still being formed at the bottom of the sea from the detritus carried down by rivers; perceiving, further, that the strata of which the visible surface chiefly consists, bore marks of having been similarly formed out of pre-existing land; and inferring that these strata could have become land only by upheaval after their deposit; concluded that throughout an indefinite past, there had been periodic convulsions, by which continents were raised, with intervening eras of repose, during which such continents were worn down and transformed into new marine strata, fated to be in their turns elevated above the surface of the ocean. And finding that igneous action, to which sundry earlier geologists had ascribed basaltic rocks, was in countless places a cause of disturbance, he taught that from it resulted these periodic convulsions. In this theory we see:—first, that the previously-recognized agency of water was conceived to act, not as by Werner, after a manner of which we have no experience, but after a manner daily displayed to us; and secondly, that the igneous agency, before considered only as originating special formations, was recognized as a universal agency, but assumed to act in an unproved way. Werner's sole process Hutton developed from the catastrophic and inexplicable into the uniform and explicable; while that antagonistic second process, of which he first adequately estimated the importance, was regarded by him as a catastrophic one, and was not assimilated to known processes—not explained. We have here to note, however, that the facts collected and provisionally arranged in conformity with Werner's theory, served, after a time, to establish Hutton's more rational theory—in so far, at least, as aqueous formations are concerned; while the doctrine of periodic subterranean convulsions, crudely as it was conceived by Hutton, was a temporary generalization needful as a step towards the theory of igneous action.

Since Hutton's time, the development of geological thought has gone still further in the same direction. These early sweeping doctrines have received additional qualifications. It has been discovered that more numerous and more heterogeneous agencies have been at work, than was at first believed. The conception of igneous action has been rationalized, as the conception of aqueous action had previously been. The gratuitous assumption that vast elevations suddenly occurred after long intervals of quiescence, has grown into the consistent theory, that islands and continents are the accumulated results of successive small upheavals, like those experienced in ordinary earthquakes. To speak more specifically, we find, in the first place, that instead of assuming the denudation produced by rain and rivers to be the sole means of wearing down lands and producing their irregularities of surface, geologists now see that denudation is only a part-cause of such irregularities; and further, that the new strata deposited at the bottom of the sea, are not the products of river-sediment solely, but are in part due to the actions of waves and tidal currents on the coasts. In the second place, we find that Hutton's conception of upheaval by subterranean forces, has not only been modified by assimilating these subterranean forces to ordinary earthquake-forces; but modern inquiries have shown that, besides elevations of surface, subsidences are thus produced; that local upheavals, as well as the general upheavals which raise continents, come within the same category; and that all these changes are probably consequent on the progressive collapse of the Earth's crust upon its cooling and contracting nucleus. In the third place, we find that beyond these two great antagonistic agencies, modern Geology recognizes sundry minor ones: those of glaciers and icebergs, those of coral-polypes; those of *Protozoa* having siliceous or calcareous shells—each of which agencies, insignificant as it seems, is found capable of slowly working terrestrial changes of considerable magnitude. Thus, then, the recent progress of Geology has been a still further departure from primitive conceptions. Instead of one catastrophic cause, once in universal action, as supposed by Werner—instead of one general continuous cause, antagonized at long intervals by a catastrophic cause, as taught by Hutton; we now recognize several causes, all more or less general and continuous. We no longer resort to hypothetical agencies to explain the phenomena displayed by the Earth's crust; but we are day by day more clearly perceiving that these phenomena have arisen from forces like those now at work, which have acted in all varieties of combination, through immeasurable periods of time.

Having thus briefly traced the evolution of geologic science, and noted its present form, let us go on to observe the way in which it is still swayed by the crude hypotheses it set out with; so that even now, doctrines long since abandoned as untenable in theory, continue in practice to mould the ideas of geologists, and to foster sundry beliefs that are logically indefensible. We shall see, both how those simple sweeping conceptions with which the science commenced, are those which every student is apt at first to seize hold of, and how several influences conspire to maintain the twist thus resulting—how the original nomenclature of periods and formations necessarily keeps alive the original implications; and how the need for arranging new data in some order, results in their being thrust into the old classification, unless their incongruity with it is very glaring. A few facts will best prepare the way for criticism.

Up to 1839 it was inferred, from their crystalline character, that the metamorphic rocks of Anglesea were more ancient than any rocks of the adjacent main land; but it has since been shown that they are of the same age with the slates and grits of Carnarvon and Merioneth. Again, slaty cleavage having been first found only in the lowest rocks, was taken as an indication of the highest antiquity: whence resulted serious mistakes; for this mineral characteristic is now known to occur in the Carboniferous system. Once more, certain red conglomerates and grits on the north-west coast of Scotland, long supposed from their lithological aspect to belong to the Old Red Sandstone, are now identified with the Lower Silurians. These are a few instances of the small trust to be placed in mineral qualities, as evidence of the ages or relative positions of strata. From the recently-published third edition of *Siluria*, may be culled numerous facts of like implication. Sir R. Murchison considers it ascertained, that the siliceous Stiper stones of Shropshire are the equivalents of the Tremadock slates of North Wales. Judged by their fossils, Bala slate and limestone are of the same age as the Caradoc sandstone, lying forty miles off. In Radnorshire, the formation classed as upper Llandovery rock, is described at different spots, as "sandstone or conglomerate," "impure limestone," "hard coarse grits," "siliceous grit"—a considerable variation for so small an area as that of a county. Certain sandy beds on the left bank of the Towy, which Sir R. Murchison had, in his *Silurian System*, classed as Caradoc sandstone (evidently from their mineral characters), he now finds, from their fossils, belong to the Llandeilo formation. Nevertheless, inferences from mineral characters are still habitually drawn and received. Though *Siluria*, in common with other geological works, supplies numerous proofs that rocks of the same age are often of widely-different composition a few miles off, while rocks of widely-different ages are often of similar composition; and though Sir R. Murchison shows us, as in the case just cited, that he has himself in past times been misled by trusting to lithological evidence; yet his reasoning all through *Siluria*, shows that he still thinks it natural to expect formations of the same age to be chemically similar, even in remote regions. For example, in treating of the Silurian rocks of South Scotland, he says:—"When traversing the tract between Dumfries and Moffat, in 1850, it occurred to me, that the dull reddish or purple sandstone and schist to the north of the former town, which so resembled the bottom rocks of Longmynd, Llanberis, and St. David's, would prove to be of the same age;" and further on, he again insists upon the fact that these strata "are absolutely of the same composition as the bottom rocks of the Silurian region." On this unity of mineral character it is, that this Scottish formation is concluded to be contemporaneous with the lowest formations in Wales; for the scanty paleontological evidence suffices for neither proof nor disproof. Now, had there been a continuity of like strata in like order between Wales and Scotland, there might have been little to criticize in this conclusion. But since Sir R. Murchison himself admits, that in Westmoreland and Cumberland, some members of the system "assume a lithological aspect different from what they maintain in the Silurian and Welsh region," there seems no reason to expect mineralogical continuity in Scotland. Obviously, therefore, the assumption that these Scottish formations are of the same age with the Longmynd of Shropshire, implies the latent belief that certain mineral characters indicate certain eras. Far more striking instances, however, of the influence of this latent belief remain to be given. Not in such comparatively near districts as the Scottish lowlands only, does Sir R. Murchison expect a repetition of the Longmynd strata; but in the Rhenish provinces, certain "quartzose flagstones and grits, like those of the Longmynd," are seemingly concluded to be of contemporaneous origin, because of their likeness. "Quartzites in roofing-slates with a greenish tinge that reminded us of the lower slates of Cumberland and Westmoreland," are evidently suspected to be of the same age. In Russia, he remarks that the carboniferous limestones "are overlaid along the western edge of the Ural chain by sandstones and grits, which occupy much the same place in the general series as the millstone grit of England;" and in calling this group, as he does, the "representative of the millstone grit," Sir R. Murchison clearly shows that he thinks likeness of mineral composition some evidence of equivalence in time, even at that great distance. Nay, on the flanks of the Andes and in the United States, such similarities are looked for, and considered as significant of certain ages. Not that Sir R. Murchison contends theoretically for this relation between lithological character and date. For on the page from which we have just quoted (*Siluria*, p. 387), he says, that "whilst the soft Lower Silurian clays and sands of St. Petersburg have their equivalents in the hard schists and quartz rocks with gold veins in the heart of the Ural mountains, the equally soft red and green Devonian marls of the Valdai Hills are represented on the western flank of that chain by hard, contorted, and fractured limestones." But these, and other such admissions, seem to go for little. While himself asserting that the Potsdam-sandstone of North America, the Lingula-flags of England, and the alum-slates of Scandinavia are of the same period—while fully aware that among the Silurian formations of Wales, there are oolitic strata like those of secondary age; yet his reasoning is more or less coloured by the assumption, that formations of like qualities probably belong to the same era. Is it not manifest, then, that the exploded hypothesis of Werner continues to influence geological speculation?

"But," it will perhaps be said, "though individual strata are not continuous over large areas, yet systems of strata are. Though within a few miles the same bed gradually passes from clay into sand, or thins out and disappears, yet the group of strata to which it belongs does not do so; but maintains in remote regions the same relations to other groups."

This is the generally-current belief. On this assumption the received geological classifications appear to be framed. The Silurian system, the Devonian system, the Carboniferous system, etc., are set down in our books as groups of formations which everywhere succeed each other in a given order; and are severally everywhere of the same age. Though it may not be asserted that these successive systems are universal; yet it seems to be tacitly assumed that they are. In North and South America, in Asia, in Australia, sets of strata are assimilated to one or other of these groups; and their possession of certain mineral characters and a certain order of superposition are among the reasons assigned for so assimilating them. Though, probably, no competent geologist would contend that the European classification of strata is applicable to the globe as a whole; yet most, if not all geologists, write as though it were. Among readers of works on Geology, nine out of ten carry away the impression that the divisions, Primary, Secondary and Tertiary, are of absolute and uniform application; that these great divisions are separable into subdivisions, each of which is definitely distinguishable from the rest, and is everywhere recognizable by its characters as such or such; and that in all parts of the Earth, these minor systems severally began and ended at the same time. When they meet with the term "Carboniferous era," they take for granted that it was an era universally carboniferous—that it was, what Hugh Miller indeed actually describes it, an era when the Earth bore a vegetation far more luxuriant than it has since done; and were they in any of our colonies to meet with a coal-bed, they would conclude that, as a matter of course, it was of the same age as the English coal-beds.

Now this belief that geologic "systems" are universal, is no more tenable than the other. It is just as absurd when considered *a priori;* and it is equally inconsistent with the facts. Though some series of strata classed together as Oolite, may range over a wider district than any one stratum of the series; yet we have but to ask what were the circumstances under which it was deposited, to see that the Oolitic series, like one of its individual strata, must be of local origin; and that there is not likely to be anywhere else, a series which corresponds, either in its characters or in its commencement and termination. For the formation of such a series implies an area of subsidence, in which its component beds were thrown down. Every area of subsidence is necessarily limited; and to suppose that there exist elsewhere groups of beds completely answering to those known as Oolite, is to suppose that, in contemporaneous areas of subsidence, like processes were going on.

There is no reason to suppose this; but good reason to suppose the reverse. That in contemporaneous areas of subsidence throughout the globe, the conditions would cause the formation of Oolite, is an assumption which no modern geologist would openly make. He would say that the equivalent series of beds found elsewhere, would probably be of dissimilar mineral character. Moreover, in these contemporaneous areas of subsidence, the processes going on would not only be different in kind; but in no two cases would they be likely to agree in their commencements and terminations. The probabilities are greatly against separate portions of the Earth's surface beginning to subside at the same time, and ceasing to subside at the same time—a coincidence which alone could produce equivalent groups of strata. Subsidences in different places begin and end with utter irregularity; and hence the groups of strata thrown down in them can but rarely correspond. Measured against each other in time, their limits must disagree. On turning to the evidence, we find that it daily tends more and more to justify these *a priori* positions. Take, as an example, the Old Red Sandstone system. In the north of England this is represented by a single stratum of conglomerate. In Herefordshire, Worcestershire, and Shropshire, it expands into a series of strata from eight to ten thousand feet thick, made up of conglomerates, red, green, and white sandstones, red, green, and spotted marls, and concretionary limestones. To the south-west, as between Caermarthen and Pembroke, these Old Red Sandstone strata exhibit considerable lithological changes; on the other side of the Bristol Channel, they display further changes in mineral characters; while in South Devon and Cornwall, the equivalent strata, consisting chiefly of slates, schists, and limestones, are so wholly different, that they were for a long time classed as Silurian. When we thus see that in certain directions the whole group of deposits thins out, and that its mineral characters change within moderate distances; does it not become clear that the whole group of deposits was a local one? And when we find, in other regions, formations analogous to these Old Red Sandstone or Devonian formations, is it certain—is it even probable—that they severally began and ended at the same time with them? Should it not require overwhelming evidence to make us believe as much?

Yet so strongly is geological speculation swayed by the tendency to regard the phenomena as general instead of local, that even those most on their guard against it seem unable to escape its influence. At page 158 of his *Principles of Geology*, Sir Charles Lyell says:—

> "A group of red marl and red sandstone, containing salt and gypsum, being interposed in England between the Lias and the Coal, all other red marls and sandstones, associated some of them with salt, and others with gypsum, and occurring not only in different parts of Europe, but in North America, Peru, India, the salt deserts of Asia, those of Africa—in a word, in every quarter of the globe, were referred to one and the same period. . . . . . It was in vain to urge as an objection the improbability of the hypothesis which implies that all the moving waters on the globe were once simultaneously charged with sediment of a red colour. But the rashness of pretending to identify, in age, all the red sandstones and marls in question, has at length been sufficiently exposed, by the discovery that, even in Europe, they belong decidedly to many different epochs."

Nevertheless, while in this and many kindred passages Sir C. Lyell protests against the bias here illustrated, he seems himself not completely free from it. Though he utterly rejects the old hypothesis that all over the Earth the same continuous strata lie one upon another in regular order, like the coats of an onion, he still writes as though geologic "systems" do thus succeed each other. A reader of his *Manual* would certainly suppose him to believe, that the Primary epoch ended, and the secondary epoch began, all over the world at the same time—that these terms really correspond to distinct universal eras. When he assumes, as he does, that the division between Cambrian and Lower Silurian in America, answers chronologically to the division between Cambrian and Lower Silurian in

Wales—when he takes for granted that the partings of Lower from Middle Silurian, and of Middle Silurian from Upper, in the one region, are of the same dates as the like partings in the other region; does it not seem that he believes geologic "systems" to be universal, in the sense that their separations were in all places contemporaneous? Though he would, doubtless, disown this as an article of faith, is not his thinking unconsciously influenced by it? Must we not say that, though the onion-coat hypothesis is dead, its spirit is traceable, under a transcendental form, even in the conclusions of its antagonists?

Let us now consider another leading geological doctrine,—the doctrine that strata of the same age contain like fossils; and that, therefore, the age and relative position of any stratum may be known by its fossils. While the theory that strata of like mineral characters were everywhere deposited simultaneously, has been ostensibly abandoned, there has been accepted the theory that in each geologic epoch similar plants and animals existed everywhere; and that, therefore, the epoch to which any formation belongs may be known by the organic remains contained in the formation. Though, perhaps, no leading geologist would openly commit himself to an unqualified assertion of this theory, yet it is tacitly assumed in current geological reasoning.

This theory, however, is scarcely more tenable than the other. It cannot be concluded with any certainty, that formations in which similar organic remains are found, were of contemporaneous origin; nor can it be safely concluded that strata containing different organic remains are of different ages. To most readers these will be startling propositions; but they are fully admitted by the highest authorities. Sir Charles Lyell confesses that the test of organic remains must be used "under very much the same restrictions as the test of mineral composition." Sir Henry de la Beche, Yet the improbability of this assumption may be readily shown from Sir R. Murchison's own facts. If, as he points out, the fossil crustaceans of the uppermost Silurian rocks in Lanarkshire are, "with one doubtful exception," all "distinct from any of the forms known on the same horizon in England;" how can it be fairly presumed that the forms existing on the other side of the Earth during the Silurian period, were nearly allied to those existing here? Not only, indeed, do Sir R. Murchison's conclusions tacitly assume this doctrine of universal distribution, but he distinctly enunciates it. "The mere presence of a graptolite," he says, "will at once decide that the enclosing rock is Silurian;" and he says this, notwithstanding repeated warnings against such generalizations. During the progress of Geology, it has over and over again happened that a particular fossil, long considered characteristic of a particular formation, has been afterwards discovered in other formations. Until some twelve years ago, Goniatites had not been found lower than the Devonian rocks; but now, in Bohemia, they have been found in rocks classed as Silurian. Quite recently, the *Orthoceras*, previously supposed to be a type exclusively palæozoic, has been detected along with mesozoic Ammonites and Belemnites. Yet hosts of such experiences fail to extinguish the assumption, that the age of a stratum may be determined by the occurrence in it of a single fossil form. Nay, this assumption survives evidence of even a still more destructive kind. Referring to the Silurian system in Western Ireland, Sir R. Murchison says, "in the beds near Maam, Professor Nicol and myself collected remains, some of which would be considered Lower, and others Upper, Silurian;" and he then names sundry fossils which, in England, belong to the summit of the Ludlow rocks, or highest Silurian strata; "some, which elsewhere are known only in rocks of Llandovery age"—that is, of middle Silurian age; and some, only before known in Lower Silurian strata, not far above the most ancient fossiliferous beds. Now what do these facts prove? Clearly, they prove that species which in Wales are separated by strata more than twenty thousand feet deep, and therefore seem to belong to periods far more remote from each other, were really co-existent. They prove that the mollusks and crinoids held to be characteristic of early Silurian strata, and supposed to have become extinct long before the mollusks and crinoids of the later Silurian strata came into existence, were really flourishing at the same time with these last; and that these last possibly date back to as early a period as the first. They prove that not only the mineral characters of sedimentary formations, but also the collections of organic forms they contain, depend, to a great extent, on local circumstances. They prove that the fossils met with in any series of strata, cannot be taken as representing anything like the whole Flora and Fauna of the period they belong to. In brief, they throw great doubt upon numerous geological generalizations.

Notwithstanding facts like these, and notwithstanding his avowed opinion that the test of organic remains must be used "under very much the same restrictions as the test of mineral composition," Sir Charles Lyell, too, considers sundry positive conclusions to be justified by this test: even where the community of fossils is slight and the distance great. Having decided that in various places in Europe, middle Eocene strata are distinguished by Nummulites; he infers, without any other assigned evidence, that wherever Nummulites are found—in Morocco, Algeria, Egypt, in Persia, Scinde, Cutch, Eastern Bengal, and the frontiers of China—the containing formation is Middle Eocene. And from this inference he draws the following important corollary:—

> "When we have once arrived at the conviction that the nummulitic formation occupies a middle place in the Eocene series, we are struck with the comparatively modern date to which some of the greatest revolutions in the physical geography of Europe, Asia, and northern Africa must be referred. All the mountain chains, such as the Alps, Pyrenees, Carpathians, and Himalayas, into the composition of whose central and loftiest parts the

**nummulitic strata enter bodily, could have had no existence till after the Middle Eocene period."—*Manual*, p. 232.**

A still more marked case follows on the next page. Because a certain bed at Claiborne in Alabama, which contains "*four hundred* species of marine shells," includes among them the *Cardita planicosta*, "and *some others* identical with European species, or very nearly allied to them," Sir C. Lyell says it is "highly probable the Claiborne beds agree in age with the central or Bracklesham group of England." When we find contemporaneity alleged on the strength of a community no greater than that which sometimes exists between strata of widely-different ages in the same country, it seems as though the above-quoted caution had been forgotten. It appears to be assumed for the occasion, that species which had a wide range in space had a narrow range in time; which is the reverse of the fact. The tendency to systematize overrides the evidence, and thrusts Nature into a formula too rigid to fit her endless variety.

"But," it may be urged, "surely, when in different places the order of superposition, the mineral characters, and the fossils, agree, it may safely be concluded that the formations thus corresponding date back to the same time. If, for example, the United States display a succession of Silurian, Devonian, and Carboniferous systems, lithologically similar to those known here by those names, and characterized by like fossils, it is a fair inference that these groups of strata were severally being deposited in America while their equivalents were being deposited here."

On this position, which seems a strong one, we have, in the first place, to remark, that the evidence of correspondence is always more or less suspicious. We have already adverted to the several "idols"—if we may use Bacon's metaphor—to which geologists unconsciously sacrifice, when interpreting the structures of unexplored regions. Carrying with them the classification of strata existing in Europe, and assuming that groups of strata in other parts of the world must answer to some of the groups of strata known here, they are necessarily prone to assert parallelism on insufficient evidence. They scarcely entertain the previous question, whether the formations they are examining have or have not any European equivalents; but the question is—with which of the European series shall they be classed?—with which do they most agree?—from which do they differ least? And this being the mode of inquiry, there is apt to result great laxity of interpretation. How lax the interpretation really is, may be readily shown. When strata are discontinuous, as between Europe and America, no evidence can be derived from the order of superposition, apart from mineral characters and organic remains; for, unless strata can be continuously traced, mineral characters and organic remains afford the only means of classing them as such or such. As to the test of mineral characters, we have seen that it is almost worthless; and no modern geologist would dare to say it should be relied on. If the Old Red Sandstone series in mid-England, differs wholly in lithological aspect from the equivalent series in South Devon, it is clear that similarities of texture and composition cannot justify us in classing a system of strata in another quarter of the globe with some European system. The test of fossils is the only one that remains; and with how little strictness this test is applied, one case will show. Of forty-six species of British Devonian corals, only six occur in America; and this, notwithstanding the wide range which the *Anthozoa* are known to have. Similarly of the *Mollusca* and *Crinoidea*, it appears that, while there are sundry genera found in America which are found here, there are scarcely any of the same species. And Sir Charles Lyell admits that "the difficulty of deciding on the exact parallelism of the New York subdivisions, as above enumerated, with the members of the European Devonian, is very great, so few are the species in common." Yet it is on the strength of community of fossils, that the whole Devonian series of the United States is assumed to be contemporaneous with the whole Devonian series of England. And it is partly on the ground that the Devonian of the United States corresponds in time with our own Devonian, that Sir Charles Lyell concludes the superjacent coal-measures of the two countries to be of the same age. Is it not, then, as we said, that the evidence in these cases is very suspicious? Should it be replied, as it may fairly be, that this correspondence from which the synchronism of distant formations is inferred, is not a correspondence between particular species or particular genera, but between the general characters of the contained assemblages of fossils—between the *facies* of the two Faunas; the rejoinder is, that though such correspondence is a stronger evidence of synchronism it is still an insufficient one. To infer synchronism from such correspondence, involves the postulate that throughout each geologic era there has habitually existed a recognizable similarity between the groups of organic forms inhabiting all the different parts of the Earth; and that the causes which have in one part of the Earth changed the organic forms into those which characterize the next era, have simultaneously acted in all other parts of the Earth, in such ways as to produce parallel changes of their organic forms. Now this is not only a large assumption to make; but it is an assumption contrary to probability. The probability is, that the causes which have changed Faunas have been local rather than universal; that hence while the Faunas of some regions have been rapidly changing, those of others have been almost quiescent; and that when those of others have been changed, it has been, not in such ways as to maintain parallelism, but in such ways as to produce divergence.

Even supposing, however, that districts some hundreds of miles apart, furnished groups of strata which completely agreed in their order of superposition, their mineral characters, and their fossils, we should still have inadequate proof of contemporaneity. For there are conditions, very likely to occur, under which such groups might differ widely in age. If there be a continent of which the strata crop out on the surface obliquely to the line of coast—running, say, west-north-west, while the coast runs east and west—it is clear that each group of strata will crop out on the beach at a particular part of the coast; that further west the next group of strata will crop out on the beach; and so continuously. As the localization of marine plants and animals, is in a considerable degree determined by the natures of the rocks and their detritus, it follows that each part of this coast will have its more or less distinct Flora and Fauna. What now must result from the action of the waves in the course of a geologic epoch? As the sea makes slow inroads on the land, the place at which each group of strata crops out on the beach will gradually move towards the west: its distinctive fish, mollusks, crustaceans, and sea-weeds, migrating with it. Further, the detritus of each of these groups of strata will, as the point of outcrop moves westwards, be deposited over the detritus of the group in advance of it. And the consequence of these actions, carried on for one of those enormous periods which a geologic change takes, will be that, corresponding to each eastern stratum, there will arise a stratum far to the west, which, though occupying the same position relatively to other beds, formed of like materials, and containing like fossils, will yet be perhaps a million years later in date.

But the illegitimacy, or at any rate the great doubtfulness, of many current geological inferences, is best seen when we contemplate terrestrial changes now going on; and ask how far such inferences are countenanced by them. If we carry out rigorously the modern method of interpreting geological phenomena, which Sir Charles Lyell has done so much to establish—that of referring them to causes like those at present in action—we cannot fail to see how improbable are sundry of the received conclusions.

Along each shore which is being worn away by the waves, there are being formed mud, sand, and pebbles. This detritus has, in each locality, a more or less special character; determined by the nature of the strata destroyed. In the English Channel it is not the same as in the Irish Channel; on the east coast of Ireland it is not the same as on the west coast; and so throughout. At the mouth of each great river, there is being deposited sediment differing more or less from that deposited at the mouths of other rivers in colour and quality; forming strata which are here red, there yellow, and elsewhere brown, grey, or dirty white. Besides which various formations, going on in deltas and along shores, there are some much wider, and still more strongly contrasted, formations. At the bottom of the Ægean Sea, there is accumulating a bed of Pteropod shells, which will eventually, no doubt, become a calcareous rock. For some hundreds of thousands of square miles, the ocean-bed between Great Britain and North America, is being covered with a stratum of chalk; and over large areas in the Pacific, there are going on deposits of coralline limestone. Thus, there are at this moment being produced in different places multitudinous strata differing from one another in lithological characters. Name at random any part of the sea-bottom, and ask whether the deposit there taking place is like the deposit taking place at some distant part of the sea-bottom, and the almost-certainly correct answer will be—No. The chances are not in favour of similarity, but against it—many to one against it.

In the order of superposition of strata there is being established a like variety. Each region of the Earth's surface has its special history of elevations, subsidences, periods of rest: and this history in no case fits chronologically with the history of any other portion. River deltas are now being thrown down on formations of different ages: some very ancient, some quite modern. While here there has been deposited a series of beds many hundreds of feet thick, there has elsewhere been deposited but a single bed of fine mud. While one region of the Earth's crust, continuing for a vast epoch above the surface of the ocean, bears record of no changes save those resulting from denudation; another region of the Earth's crust gives proof of sundry changes of level, with their several resulting masses of stratified detritus. If anything is to be judged from current processes, we must infer, not only that everywhere the succession of sedimentary formations differs more or less from the succession elsewhere; but also that in each place, there exist groups of strata to which many other places have no equivalents.

With respect to the organic bodies imbedded in formations now in progress, a like truth is equally manifest, if not more manifest. Even along the same coast, within moderate distances, the forms of life differ very considerably; and they differ much more on coasts that are remote from one another. Again, dissimilar creatures which are living together near the same shore, do not leave their remains in the same beds of sediment. For instance, at the bottom of the Adriatic, where the prevailing currents cause the deposits to be here of mud, and there of calcareous matter, it is proved that different species of co-existing shells are being buried in these respective formations. On our own coasts, the marine remains found a few miles from shore, in banks where fish congregate, are different from those found close to the shore, where littoral species flourish. A large proportion of aquatic creatures have structures which do not admit of fossilization; while of the rest, the great majority are destroyed, when dead, by various kinds of scavengers. So that no one deposit near our shores can contain anything like a true representation of the Fauna of the surrounding sea; much less of the co-existing Faunas of other seas in the same latitude; and still less of the Faunas of seas in distant latitudes. Were it not that the assertion seems needful, it would be almost absurd to say, that the organic remains now being buried in the Dogger Bank, can tell us next to nothing about the fish, crustaceans, mollusks, and corals, which are being buried in the Bay of Bengal. Still stronger is the argument in the case of terrestrial life. With more numerous and greater contrasts between the types inhabiting one continent and those inhabiting another, there is a far more imperfect registry of them. Schouw marks out on the Earth more than twenty botanical regions, occupied by groups of forms so distinct, that, if fossilized, geologists would scarcely be disposed to refer them all to the same period. Of Faunas, the Arctic differs from the Temperate; the Temperate from the Tropical; and the South Temperate from the North Temperate. Nay, in the South Temperate Zone itself, the two regions of South Africa and South America are unlike in their mammals, birds, reptiles, fishes, mollusks, insects. The shells and bones now lying at the bottoms of lakes and estuaries in these several regions, have certainly not that similarity which is usually looked for in those of contemporaneous strata; and the recent forms exhumed in any one of these regions would very untruly represent the present Flora and Fauna of the Earth. In conformity with the current style of geological reasoning, an exhaustive examination of deposits in the Arctic circle, might be held to prove that though at this period there were sundry mammals existing, there were no reptiles; while the absence of mammals in the deposits of the Galapagos Archipelago, where there are plenty of reptiles, might be held to prove the reverse. And at the same time, from the formations extending for two thousand miles along the great barrier-reef of Australia—formations in which are imbedded nothing but corals, echinoderms, mollusks, crustaceans, and fish, along with an occasional turtle, or bird, or cetacean—it might be inferred that there lived in our epoch neither terrestrial reptiles, nor terrestrial mammals. The mention of Australia, indeed, suggests an illustration which, even alone, would amply prove our case. The Fauna of this region differs widely from any that is found elsewhere. On land, all the indigenous mammals, except bats, belong to the lowest, or implacental division; and the insects are singularly different from those found elsewhere. The surrounding seas contain numerous forms which are more or less strange; and among the fish there exists a species of shark, which is the only living representive of a genus that flourished in early geologic epochs. If, now, the modern fossiliferous deposits of Australia were to be examined by one ignorant of the existing Australian Fauna; and if he were to reason in the usual manner; he would be very unlikely to class these deposits with those of the present time. How, then, can we place confidence in the tacit assumption that certain formations in remote parts of the Earth are referable to the same period, because the organic remains contained in them display a certain community of character? or that certain others are referable to different periods, because the *facies* of their Faunas are different?

"But," it will be replied, "in past eras the same, or similar, organic forms were more widely distributed than now." It may be so; but the evidence adduced by no means proves it. The argument by which this conclusion is reached, runs a risk of being quoted as an example of reasoning in a circle. As already pointed out, between formations in remote regions the accepted test of equivalence is community of fossils. If, then, the contemporaneity of remote formations is concluded from the likeness of their fossils; how can it be said that similar plants and animals were once more widely distributed, because they are found in contemporaneous strata in remote regions? Is not the fallacy manifest? Even supposing there were no such fatal objection as this, the evidence commonly assigned would still be insufficient. For we must bear in mind that the community of organic remains usually thought sufficient proof of correspondence in time, is a very imperfect community. When the compared sedimentary beds are far apart, it is scarcely expected that there will be many species common to the two: it is enough if there be discovered a considerable number of common genera. Now had it been proved that throughout geologic time, each genus lived but for a short period—a period measured by a single group of strata—something might be inferred. But what if we learn that many of the same genera continued to exist throughout enormous epochs, measured by several vast systems of strata? "Among molluscs, the genera *Avicula, Modiola, Terebratula, Lingula,* and *Orbicula,* are found from the Silurian rocks upwards to the present day." If, then, between the lowest fossiliferous formations and the most recent, there exists this degree of community; must we not infer that there will probably often exist a great degree of community between strata that are far from contemporaneous?

Thus the reasoning from which it is concluded that similar organic forms were once more widely spread than now, is doubly fallacious; and, consequently, the classifications of foreign strata based on the conclusion are untrustworthy. Judging from the present distribution of life, we cannot expect to find similar remains in geographically remote strata of the same age; and where, between the fossils of geographically remote strata, we do find much similarity, it is probably due rather to likeness of conditions than to contemporaneity. If from causes and effects such as we now witness, we reason back to the causes and effects of past epochs, we discover inadequate warrant for sundry of the received doctrines. Seeing, as we do, that in large areas of the Pacific this is a period characterized by abundance of corals; that in the North Atlantic it is a period in which a great chalk-deposit is being formed; and that in the valley of the Mississippi it is a period of new coal-basins—seeing also, as we do, that in one extensive continent this is peculiarly an era of implacental mammals, and that in another extensive continent it is peculiarly an era of placental mammals; we have good reason to hesitate before accepting these sweeping generalizations which are based on a cursory examination of strata occupying but a tenth part of the Earth's surface.

At the outset, this article was to have been a review of the works of Hugh Miller; but it has grown into something much more general. Nevertheless, the remaining two doctrines which we propose to criticize, may conveniently be treated in connexion with his name, as that of one who fully committed himself to them. And first, a few words respecting his position.

That he was a man whose life was one of meritorious achievement, every one knows. That he was a diligent and successful working geologist, scarcely needs saying. That with indomitable perseverance he struggled up from obscurity to a place in the world of literature and science, shows him to have been highly endowed in character and intelligence. And that he had a remarkable power of presenting his facts and arguments in an attractive form, a glance at any of his books will quickly prove. By all means, let us respect him as a man of activity and sagacity, joined with a large amount of poetry. But while saying this we must add, that his reputation stands by no means so high in the scientific world as in the world at large. Partly from the fact that our Scotch neighbours are in the habit of blowing the trumpet rather loudly before their notabilities—partly because the charming style in which his books are written has gained him a large circle of readers—partly, perhaps, through a praiseworthy sympathy with him as a self-made man; Hugh Miller has met with an amount of applause which, little as we wish to diminish it, must not be allowed to blind the public to his defects as a man of science. The truth is, he was so far committed to a foregone conclusion, that he could not become a philosophical geologist. He might be aptly described as a theologian studying geology. The dominant idea with which he wrote, may be seen in the titles of two of his books—*Footprints of the Creator*,—*The Testimony of the Rocks*. Regarding geological facts as evidence for or against certain religious conclusions, it was scarcely possible for him to deal with geological facts impartially. His ruling aim was to disprove the Development Hypothesis, the assumed implications of which were repugnant to him; and in proportion to the strength of his feeling, was the one-sidedness of his reasoning. He admitted that "God might as certainly have *originated* the species by a law of development, as he *maintains* it by a law of development;—the existence of a First Great Cause is as perfectly compatible with the one scheme as with the other." Nevertheless, he considered the hypothesis at variance with Christianity; and therefore combated with it. He apparently overlooked the fact, that the doctrines of geology in general, as held by himself, had been rejected by many on similar grounds; and that he had himself been repeatedly attacked for his anti-Christian teachings. He seems not to have perceived that, just as his antagonists were wrong in condemning as irreligious, theories which he saw were not irreligious; so might he be wrong in condemning, on like grounds, the Theory of Evolution. In brief, he fell short of that highest faith which knows that all truths must harmonize; and which is, therefore, content trustfully to follow the evidence whithersoever it leads.

Of course it is impossible to criticize his works without entering on this great question to which he chiefly devoted himself. The two remaining doctrines to be here discussed, bear directly on this question; and, as above said, we propose to treat them in connexion with Hugh Miller's name, because, throughout his reasonings, he assumes their truth. Let it not be supposed, however, that we shall aim to prove what he has aimed to disprove. While we purpose showing that his geological arguments against the Development Hypothesis are based on invalid assumptions; we do not purpose showing that the geological arguments urged in support of it are based on valid assumptions. We hope to make it apparent that the geological evidence at present obtained, is insufficient for either side; further, that there seems little probability that sufficient evidence will ever be obtained; and that if the question is eventually decided, it must be decided on other than geological grounds.

The first of the current doctrines to which we have just referred, is, that there occur in the serial records of former life on our planet, two great blanks; whence it is inferred that, on at least two occasions, the previously existing inhabitants of the Earth were almost wholly destroyed, and a different class of inhabitants created. Comparing the general life on the Earth to a thread, Hugh Miller says:—

> "It is continuous from the present time up to the commencement of the Tertiary period; and then so abrupt a break occurs, that, with the exception of the microscopic diatomaceæ, to which I last evening referred, and of one shell and one coral, not a single species crossed the gap. On its farther or remoter side, however, where the Secondary division closes, the intermingling of species again begins, and runs on till the commencement of this great Secondary division; and then, just where the Palæozoic division closes, we find another abrupt break, crossed, if crossed at all,—for there still exists some doubt on the subject,—by but two species of plant."

These breaks are supposed to imply actual new creations on the surface of our planet—supposed not by Hugh Miller only, but by the majority of geologists. And the terms Palæozoic, Mesozoic, and Cainozoic, are used to indicate these three successive systems of life. It is true that some accept this belief with caution; knowing how geologic research has been all along tending to fill up what were once thought wide gaps. Sir Charles Lyell points out that "the hiatus which exists in Great Britain between the fossils of the Lias and those of the Magnesian Limestone, is supplied in Germany by the rich fauna and flora of the Muschelkalk, Keuper, and Bunter Sandstein, which we know to be of a date precisely intermediate." Again he remarks that "until lately the fossils of the coal-measures were separated from those of the antecedent Silurian group by a very abrupt and decided line of demarcation; but recent discoveries have brought to light in Devonshire, Belgium, the Eifel, and Westphalia, the remains of a fauna of an intervening period." And once more, he says, "we have also in like manner had some success of late years in diminishing the hiatus which still separates the Cretaceous and Eocene periods in Europe." To which let us add that, since Hugh Miller penned the passage above quoted, the second of the great gaps he refers to has been very considerably narrowed by the discovery of strata containing Palæozoic genera and Mesozoic genera intermingled. Nevertheless, the occurrence of two great revolutions in the Earth's Flora and Fauna appears still to be held by many; and geologic nomenclature habitually assumes it.

Before seeking a solution of the problem thus raised, let us glance at the several minor causes which produce breaks in the geological succession of organic forms; taking first, the more general ones which modify climate, and, therefore, the distribution of life. Among these may be noted one which has not, we believe, been named by writers on the subject. We mean that resulting from a certain slow astronomic rhythm, by which the northern and southern hemispheres are alternately subject to greater extremes of temperature. In consequence of the slight ellipticity of its orbit, the Earth's distance from the sun varies to the extent of some 3,000,000 of miles. At present, the aphelion occurs at the time of our northern summer; and the perihelion during the summer of the southern hemisphere. In consequence, however, of that slow movement of the Earth's axis which produces the precession of the equinoxes, this state of things will in time be reversed:

the Earth will be nearest to the sun during the summer of the northern hemisphere, and furthest from it during the southern summer or northern winter. The period required to complete the slow movement producing these changes, is nearly 26,000 years; and were there no modifying process, the two hemispheres would alternately experience this coincidence of summer with relative nearness to the sun, during a period of 13,000 years. But there is also a still slower change in the direction of the axis major of the Earth's orbit; from which it results that the alternation we have described is completed in about 21,000 years. That is to say, if at a given time the Earth is nearest to the sun at our mid-summer, and furthest from the sun at our mid-winter; then, in 10,500 years afterwards, it will be furthest from the sun at our mid-summer, and nearest at our mid-winter. Now the difference between the distances from the sun at the two extremes of this alternation, amounts to one-thirtieth; and hence, the difference between the quantities of heat received from the sun on a summer's day under these opposite conditions amounts to one-fifteenth. Estimating this, not with reference to the zero of our thermometers, but with reference to the temperature of the celestial spaces, Sir John Herschel calculates "23° Fahrenheit, as the least variation of temperature under such circumstances which can reasonably be attributed to the actual variation of the sun's distance." Thus, then, each hemisphere has at a certain epoch, a short summer of extreme heat, followed by a long and very cold winter. Through the slow change in the direction of the Earth's axis, these extremes are gradually mitigated. And at the end of 10,500 years, there is reached the opposite state—a long and moderate summer, with a short and mild winter. At present, in consequence of the predominance of sea in the southern hemisphere, the extremes to which its astronomical conditions subject it, are much ameliorated; while the great proportion of land in the northern hemisphere, tends to exaggerate such contrast as now exists in it between winter and summer: whence it results that the climates of the two hemispheres are not widely unlike. But 10,000 years hence, the northern hemisphere will undergo annual variations of temperature far more marked than now.

In the last edition of his *Outlines of Astronomy*, Sir John Herschel recognizes this as an element in geological processes; regarding it as possibly a part-cause of those climatic changes indicated by the records of the Earth's past. That it has had much to do with those larger changes of climate of which we have evidence, seems unlikely, since there is reason to think that these have been far slower and more lasting; but that it must have entailed a rhythmical exaggeration and mitigation of the climates otherwise produced, seems beyond question. And it seems also beyond question that there must have been a consequent rhythmical change in the distribution of organisms—a rhythmical change to which we here wish to draw attention, as one cause of minor breaks in the succession of fossil remains. Each species of plant and animal has certain limits of heat and cold within which only it can exist; and these limits in a great degree determine its geographical position. It will not spread north of a certain latitude, because it cannot bear a more northern winter, nor south of a certain latitude, because the summer heat is too great; or else it is indirectly restrained from spreading further by the effect of temperature on the humidity of the air, or on the distribution of the organisms it lives upon. But now, what will result from a slow alteration of climate, produced as above decribed? Supposing the period we set out from is that in which the contrast of seasons is least marked, it is manifest that during the progress towards the period of most violent contrast, each species of plant and animal will gradually change its limits of distribution—will be driven back, here by the winter's increasing cold, and there by the summer's increasing heat—will retire into those localities that are still fit for it. Thus during 10,000 years, each species will ebb away from certain regions it was inhabiting; and during the succeeding 10,000 years will flow back into those regions. From the strata there forming, its remains will disappear; they will be absent from some of the superposed strata; and will be found in strata higher up. But in what shapes will they re-appear? Exposed during the 21,000 years of their slow recession and their slow return, to changing conditions of life, they are likely to have undergone modifications; and will probably re-appear with slight differences of constitution and perhaps of form—will be new varieties or perhaps new sub-species.

To this cause of minor breaks in the succession of organic forms—a cause on which we have dwelt because it has not been taken into account—we must add sundry others. Besides these periodically-recurring changes of climate, there are the irregular ones produced by re-distributions of land and sea; and these, sometimes less, sometimes greater, in degree, than the rhythmical changes, must, like them, cause in each region emigrations and immigrations of species; and consequent breaks, small or large as the case may be, in the paleontological series. Other and more special geological changes must produce other and more local blanks in the succession. By some inland elevation the natural drainage of a continent is modified; and instead of the sediment previously brought down to the sea by it, a great river brings down sediment unfavourable to various plants and animals living in its delta: whereupon these disappear from the locality, perhaps to re-appear in a changed form after a long epoch. Upheavals or subsidences of shores or sea-bottoms, involving deviations of marine currents, remove the habitats of many species to which such currents are salutary or injurious; and further, this redistribution of currents alters the places of sedimentary deposits, and thus stops the burying of organic remains in some localities, while commencing it in others. Had we space, many more such causes of blanks in our paleontological records might be added. But it is needless here to enumerate them. They are admirably explained and illustrated in Sir Charles Lyell's *Principles of Geology*.

Now, if these minor changes of the Earth's surface produce minor breaks in the series of fossilized remains; must not great changes produce great breaks? If a local upheaval or subsidence causes throughout its small area the absence of some links in the chain of fossil forms; does it not follow that an upheaval or subsidence extending over a large part of the Earth's surface, must cause the absence of a great number of such links throughout a very wide area?

When during a long epoch a continent, slowly sinking, gives place to a far-spreading ocean some miles in depth, at the bottom of which no deposits from rivers or abraded shores can be thrown down; and when, after some enormous period, this ocean-bottom is gradually elevated and becomes the site for new strata; it is clear that the fossils contained in these new strata are likely to have but little in common with the fossils of the strata below them. Take, in illustration, the case of the North Atlantic. We have already named the fact that between this country and the United States, the ocean-bottom is being covered with a deposit of chalk—a deposit which has been forming, probably, ever since there occurred that great depression of the Earth's crust from which the Atlantic resulted in remote geologic times. This chalk consists of the minute shells of *Foraminifera*, sprinkled with remains of small *Entomostraca*, and probably a few Pteropod-shells; though the sounding lines have not yet brought up any of these last. Thus, in so far as all high forms of life are concerned, this new chalk-formation must be a blank. At rare intervals, perhaps, a polar bear, drifted on an iceberg, may have its bones scattered over the bed; or a dead, decaying whale may similarly leave traces. But such remains must be so rare, that this new chalk-formation, if accessible, might be examined for a century before any of them were disclosed. If now, some millions of years hence, the Atlantic-bed should be raised, and estuary deposits or shore deposits laid upon it, these would contain remains of a Flora and a Fauna so distinct from everything below them, as to appear like a new creation.

Thus, along with continuity of life on the Earth's surface, there not only *may* be, but there *must* be, great gaps in the series of fossils; and hence these gaps are no evidence against the doctrine of Evolution.

One other current assumption remains to be criticized; and it is the one on which, more than on any other, depends the view taken respecting the question of development.

From the beginning of the controversy, the arguments for and against have turned upon the evidence of progression in organic forms, found in the ascending series of our sedimentary formations. On the one hand, those who contend that higher organisms have been evolved out of lower, joined with those who contend that successively higher organisms have been created at successively later periods, appeal for proof to the facts of Paleontology; which, they say, countenance their views. On the other hand, the Uniformitarians, who not only reject the hypothesis of development, but deny that the modern forms of life are higher than the ancient ones, reply that the paleontological evidence is at present very incomplete; that though we have not yet found remains of highly-organized creatures in strata of the greatest antiquity, we must not assume that no such creatures existed when those strata were deposited; and that, probably, search will eventually disclose them.

It must be admitted that thus far, the evidence has gone in favour of the latter party. Geological discovery has year after year shown the small value of negative facts. The conviction that there are no traces of higher organisms in earlier strata, has resulted not from the absence of such traces, but from incomplete examination. At p. 460 of his *Manual of Elementary Geology*, Sir Charles Lyell gives a list in illustration of this. It appears that in 1709, fishes were not known lower than the Permian system. In 1793 they were found in the subjacent Carboniferous system; in 1828 in the Devonian; in 1840 in the Upper Silurian. Of reptiles, we read that in 1710 the lowest known were in the Permian; in 1844 they were detected in the Carboniferous; and in 1852 in the Upper Devonian. While of the Mammalia the list shows that in 1798 none had been discovered below the Middle Eocene: but that in 1818 they were discovered in the Lower Oolite; and in 1847 in the Upper Trias.

The fact is, however, that both parties set out with an inadmissible postulate. Of the Uniformitarians, not only such writers as Hugh Miller, but also such as Sir Charles Lyell,* reason as though we had found the earliest, or something like the earliest, strata. Their antagonists, whether defenders of the Development Hypothesis or simply Progressionists, almost uniformly do the like. Sir R. Murchison, who is a Progressionist, calls the lowest fossiliferous strata, "Protozoic." Prof. Ansted uses the same term. Whether avowedly or not, all the disputants stand on this assumption as their common ground.

Yet is this assumption indefensible, as some who make it very well know. Facts may be cited against it which show that it is a more than questionable one—that it is a highly improbable one; while the evidence assigned in its favour will not bear criticism.

Because in Bohemia, Great Britain, and portions of North America, the lowest unmetamorphosed strata yet discovered, contain but slight traces of life, Sir R. Murchison conceives that they were formed while yet few, if any, plants or animals had been created; and, therefore, classes them as "Azoic." His own pages, however, show the illegitimacy of the conclusion that there existed at that period no considerable amount of life. Such traces of life as have been found in the Longmynd rocks, for many years considered unfossiliferous, have been found in some of the lowest beds; and the twenty thousand feet of superposed beds, still yield no organic remains. If now these superposed strata throughout a depth of four miles, are without fossils, though the strata over which they lie prove that life had commenced; what becomes of Sir R. Murchison's inference? At page 189 of *Siluria,* a still more conclusive fact will be found. The "Glengariff grits," and other accompanying strata there described as 13,500 feet thick, contain no signs of contemporaneous life. Yet Sir R. Murchison refers them to the Devonian period—a period which had a large and varied marine Fauna. How then, from the absence of fossils in the Longmynd beds and their equivalents, can we conclude that the Earth was "azoic" when they were formed?

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* Sir Charles Lyell is no longer to be classed among Uniformitarians. With rare and admirable candour he has, since this was written, yielded to the arguments of Mr. Darwin.

"But," it may be asked, "if living creatures then existed, why do we not find fossiliferous strata of that age, or an earlier age?" One reply is, that the non-existence of such strata is but a negative fact—we have not found them. And considering how little we know even of the two-fifths of the Earth's surface now above the sea, and how absolutely ignorant we are of the three-fifths below the sea, it is rash to say that no such strata exist. But the chief reply is, that these records of the Earth's earlier history have been in great part destroyed, by agencies which are ever tending to destroy such records.

It is an established geological doctrine, that sedimentary strata are liable to be changed, more or less profoundly, by igneous action. The rocks originally classed as "transition," because they were intermediate in character between the igneous rocks found below them, and the sedimentary strata found above them, are now known to be nothing else than sedimentary strata altered in texture and appearance by the intense heat of adjacent molten matter; and hence are renamed "metamorphic rocks." Modern researches have shown, too, that these metamorphic rocks are not, as was once supposed, all of the same age. Besides primary and secondary strata which have been transformed by igneous action, there are similarly-changed deposits of tertiary origin—deposits changed, even as far as a quarter of a mile from the point of contact with neighbouring granite. By this process fossils are of course destroyed. "In some cases," says Sir Charles Lyell, "dark limestones, replete with shells and corals, have been turned into white statuary marble, and hard clays, containing vegetable or other remains, into slates called mica-schist or hornblende-schist; every vestige of the organic bodies having been obliterated." Again, it is fast becoming an acknowledged truth that igneous rock, of whatever kind, is the product of sedimentary strata which have been completely melted. Granite and gneiss, which are of like chemical composition, have been shown, in various cases, to pass one into the other; as at Valorsine, near Mont Blanc, where the two, in contact, are observed to "both undergo a modification of mineral character. The granite still remaining unstratified, becomes charged with green particles; and the talcose gneiss assumes a granitiform structure without losing its stratification." In the Aberdeen-granite, lumps of unmelted gneiss are abundant; and we can ourselves bear witness that the granite on the banks of Loch Sunart yields proofs that, when molten, it contained incompletely-fused clots of sedimentary strata. Nor is this all. Fifty years ago, it was thought that all granitic rocks were primitive, or existed before any sedimentary strata; but it is now "no easy task to point out a single mass of granite demonstrably more ancient than all the known fossiliferous deposits." In brief, accumulated evidence shows, that by contact with, or proximity to, the molten matter of the Earth's nucleus, all beds of sediment are liable to be actually melted, or partially fused, or so heated as to agglutinate their particles; and that according to the temperature they have been raised to, and the circumstances under which they cool, they assume the forms of granite, porphyry, trap, gneiss, or rock otherwise altered. Further, it is manifest that though strata of various ages have been thus changed, yet the most ancient strata have been so changed to the greatest extent; both because they have been nearer to the centre of igneous agency; and because they have been for longer periods liable to be affected by it. Whence it follows, that sedimentary strata passing a certain antiquity, are unlikely to be found in an unmetamorphosed state; and that strata much earlier than these are certain to have been melted up. Thus if, throughout a past of indefinite duration, there had been at work those aqueous and igneous agencies which we see still at work, the state of the Earth's crust might be just what we find it. We have no evidence which puts a limit to the period throughout which this formation and destruction of strata has been going on. For aught the facts prove, it may have been going on for ten times the period measured by our whole series of sedimentary deposits.

Besides having, in the present appearances of the Earth's crust, no data for fixing a commencement to these processes—besides finding that the evidence permits us to assume such commencement to have been inconceivably remote, as compared even with the vast eras of geology; we are not without positive grounds for inferring the inconceivable remoteness of such commencement. Modern geology has established truths which are irreconcilable with the belief that the formation and destruction of strata began when the Cambrian rocks were formed; or at anything like so recent a time. One fact from *Siluria* will suffice. Sir R. Murchison estimates the vertical thickness of Silurian strata in Wales, at from 26,000 to 27,000 feet, or about five miles; and if to this we add the vertical depth of the Cambrian strata, on which the Silurians lie conformably, there results, on the lowest computation, a total depth of some seven miles. Now it is held by geologists, that this vast series of formations must have been deposited in an area of gradual subsidence. These beds could not have been thus laid one on another in regular order, unless the Earth's crust had been at that place sinking, either continuously or by small steps. Such an immense subsidence, however, must have been impossible without a crust of great thickness. The Earth's molten nucleus tends ever, with enormous force, to assume the form of a regular oblate spheroid. Any depression of its crust below the surface of equilibrium, and any elevation of its crust above that surface, have to withstand immense resistances. It follows inevitably that, with a thin crust, nothing but small elevations and subsidences would have been possible; and that, conversely, a subsidence of seven miles implies a crust of great strength, or, in other words, of great thickness. Indeed, if we compare this inferred subsidence in the Silurian period, with such elevations and depressions as our existing continents and oceans display, we see no evidence that the Earth's crust was appreciably thinner then than now. What are the implications? If, as geologists generally admit, the Earth's crust has resulted from that slow cooling which is even still going on—if we see no sign that at the time when the earliest Cambrian strata were formed, this crust was appreciably thinner than now; we are forced to conclude that the era during which it acquired that great thickness possessed in the Cambrian period, was enormous as compared with the interval between the Cambrian period and our own. But during the incalculable series of epochs thus implied, there existed an ocean, tides, winds, waves, rain, rivers. The agencies by which the denudation of continents and filling up of seas have all along been carried on, were as active then as now. Endless successions of strata must have been formed. And when we ask—Where are they? Nature's obvious reply is—They have been destroyed by that igneous action to which so great a part of our oldest-known strata owe their fusion or metamorphosis.

Only the last chapter of the Earth's history has come down to us. The many previous chapters, stretching back to a time immeasurably remote, have been burnt; and with them all the records of life we may presume they contained. The greater part of the evidence which might have served to settle the Development-controversy, is for ever lost; and on neither side can the arguments derived from Geology be conclusive.

"But how happen there to be such evidences of progression as exist?" it may be asked. "How happens it that, in ascending from the most ancient strata to the most recent strata, we *do* find a succession of organic forms, which, however irregularly, carries us from lower to higher?" This question seems difficult to answer. Nevertheless, there is reason for thinking that nothing can be safely inferred from the apparent progression here cited. And the illustration which shows as much, will, we believe, also show how little trust is to be placed in certain geological generalizations that appear to be well established. With this somewhat elaborate illustration, to which we now pass, our criticisms may fitly conclude.

Let us suppose that in a region now covered by wide ocean, there begins one of those great and gradual upheavals by which new continents are formed. To be precise, let us say that in the South Pacific, midway between New Zealand and Patagonia, the sea-bottom has been little by little thrust up toward the surface, and is about to emerge. What will be the successive phenomena, geological and biological, which are likely to occur before this emerging sea-bottom has become another Europe or Asia? In the first place, such portions of the incipient land as are raised to the level of the waves, will be rapidly denuded by them: their soft substance will be torn up by the breakers, carried away by the local currents, and deposited in neighbouring deeper water. Successive small upheavals will bring new and larger areas within reach of the waves; fresh portions will each time be removed from the surfaces previously denuded; and further, some of the newly-formed strata, being elevated nearly to the level of the water, will be washed away and re-deposited. In course of time the harder formations of the upraised sea-bottom will be uncovered. These, being less easily destroyed, will remain permanently above the surface; and at their margins will arise the usual breaking down of rocks into beach-sand and pebbles. While in the slow course of this elevation, going on at the rate of perhaps two or three feet in a century, most of the sedimentary deposits produced will be again and again destroyed and reformed; there will, in those adjacent areas of subsidence which accompany areas of elevation, be more or less continuous successions of sedimentary deposits lying on the pre-existing ocean bed. And now, what will be the character of these strata, old and new? They will contain scarcely any traces of life. The deposits that had previously been slowly formed at the bottom of this wide ocean, would be sprinkled with fossils of but few species. The oceanic Fauna is not a rich one; its hydrozoa do not admit of preservation; and the hard parts of its few kinds of molluscs and crustaceans and insects are mostly fragile. Hence, when the ocean-bed was here and there raised to the surface—when its strata of sediment with their contained organic fragments were torn up and long washed about by the breakers before being re-deposited—when the re-deposits were again and again subject to this violent abrading action by subsequent small elevations, as they would mostly be; what few fragile organic remains they contained, would be in nearly all cases destroyed. Thus such of the first-formed strata as survived the repeated changes of level, would be practically "azoic;" like the Cambrian of our geologists. When by the washing away of the soft deposits, the hard sub-strata had been exposed in the shape of rocky islets, and a footing had thus been furnished, the pioneers of a new life might be expected to make their appearance. What would they be? Not any of the surrounding oceanic species, for these are not fitted for a littoral life; but species flourishing on some of the far-distant shores of the Pacific. Of such, the first to establish themselves would be sea-weeds and zoophytes; because the most readily conveyed on floating wood, &c., and because when conveyed they would find fit food. It is true that Cirrhipeds and Lamellibranchs, subsisting on the minute creatures which everywhere people the sea, would also find fit food. But the chances of early colonization are in favour of species which, multiplying by agamogenesis, can people a whole shore from a single germ; and against species which, multiplying only by gamogenesis, must be introduced in considerable numbers that some may propagate. Thus we infer that the earliest traces of life left in the sedimentary deposits near these new shores, will be traces of life as humble as that indicated in the most ancient rocks of Great Britain and Ireland. Imagine now that the processes above indicated, continue—that the emerging lands become wider in extent, and fringed by higher and more varied shores; and that there still go on those ocean-currents which, at long intervals, convey from far distant shores immigrant forms of life. What will result? Lapse of time will of course favour the introduction of such new forms: admitting, as it must, of those combinations of fit conditions, which can occur only after long intervals. Moreover, the increasing area of the islands, individually and as a group, implies increasing length of coast, and therefore a longer line of contact with the streams and waves which bring drifting masses bearing germs of fresh life. And once more, the comparatively-varied shores, presenting physical conditions which change from mile to mile, will furnish suitable habitats for more numerous species. So that as the elevation proceeds, three causes conspire to introduce additional marine plants and animals. To what classes will the increasing Fauna be for a long period confined? Of course, to classes of which individuals, or their germs, are most liable to be carried far away from their native shores by floating sea-weed or drift-wood; to classes which are also least likely to perish in transit, or from change of climate; and to those which can best subsist around coasts comparatively bare of life. Evidently then, corals, annelids, inferior molluscs, and crustaceans of low grade, will chiefly constitute the early Fauna. The large predatory members of these classes, will be later in establishing themselves; both because the new shores must first become well peopled by the creatures they prey on, and because, being more complex, they, or their ova, must be less likely to survive the journey, and the change of conditions. We may infer, then, that the strata deposited next after the almost "azoic" strata, would contain the remains of invertebrata, allied to those found near the shores of Australia and South America. Of such invertebrate remains, the lower beds would furnish comparatively few genera, and those of relatively low types; while in the upper beds the number of genera would be greater, and the types higher: just as among the fossils of our Silurian system. As this great geologic change slowly advanced through its long history of earthquakes, volcanic disturbances, minor upheavals and subsidences—as the extent of the archipelago became greater and its smaller islands coalesced into larger ones, while its coast-line grew still longer and more varied, and the neighbouring sea more thickly inhabited by inferior forms of life; the lowest division of the vertebrata would begin to be represented. In order of time, fish would naturally come later than the lower invertebrata; both as being less likely to have their ova transported across the waste of waters, and as requiring for their subsistence a pre-existing Fauna of some development. They might be expected to make their appearance along with the predaceous crustaceans; as they do in the uppermost Silurian rocks. And here, too, let us remark, that as, during this long epoch we have been describing, the sea would have made great inroads on some of the newly-raised lands which had remained stationary; and would probably in some places have reached masses of igneous or metamorphic rocks; there might, in course of time, arise by the decomposition and denudation of such rocks, local deposits coloured with oxide of iron, like our Old Red Sandstone. And in these deposits might be buried the remains of the fish then peopling the neighbouring sea.

Meanwhile, how would the surfaces of the upheaved masses be occupied? At first their deserts of naked rocks would bear only the humblest forms of vegetal life, such as we find in grey and orange patches on our own rugged mountain sides; for these alone could flourish on such surfaces, and their spores would be the most readily transported. When, by the decay of such protophytes, and that decomposition of rock effected by them, there had resulted a fit habitat for mosses; these, of which the germs might be conveyed in drifted trees, would begin to spread. A soil having been eventually thus produced, it would become possible for plants of higher organization to find roothold; and as the archipelago and its constituent islands grew larger, and had more multiplied relations with winds and waters, such higher plants might be expected ultimately to have their seeds transferred from the nearest lands. After something like a Flora had thus colonized the surface, it would become possible for insects to exist; and of air-breathing creatures, insects would manifestly be among the first to find their way from elsewhere. As, however, terrestrial organisms, both vegetal and animal, are less likely than marine organisms to survive the accidents of transport from distant shores; it is inferable that long after the sea surrounding these new lands had acquired a varied Flora and Fauna, the lands themselves would still be comparatively bare; and thus that the early strata, like our Silurians, would afford no traces of terrestrial life. By the time that large areas had been raised above the ocean, we may fairly suppose a luxuriant vegetation to have been acquired. Under what circumstances are we likely to find this vegetation fossilized? Large surfaces of land imply large rivers with their accompanying deltas; and are liable to have lakes and swamps. These, as we know from extant cases, are favourable to rank vegetation; and afford the conditions needful for preserving it in coal-beds. Observe, then, that while in the early history of such a continent a carboniferous period could not occur, the occurrence of a carboniferous period would become probable after long-continued upheavals had uncovered large areas. As in our own sedimentary series, coal-beds would make their appearance only after there had been enormous accumulations of earlier strata charged with marine fossils.

Let us ask next, in what order the higher forms of animal life would make their appearance. We have seen how, in the succession of marine forms, there would be something like a progress from the lower to the higher: bringing us in the end to predaceous molluscs, crustaceans, and fish. What are likely to succeed fish? After marine creatures, those which would have the greatest chance of surviving the voyage would be amphibious reptiles; both because they are more tenacious of life than higher animals, and because they would be less completely out of their element. Such reptiles as can live in both fresh and salt water, like alligators; and such as are drifted out of the mouths of great rivers on floating trees, as Humboldt says the Orinoco alligators are; might be early colonists. It is manifest, too, that reptiles of other kinds would be among the first vertebrata to people the new continent. If we consider what will occur on one of those natural rafts of trees, soil, and matted vegetable matter, sometimes swept out to sea by such currents as the Mississippi, with a miscellaneous living cargo; we shall see that while the active, hot-blooded, highly-organized creatures will soon die of starvation and exposure, the inert, cold-blooded ones, which can go long without food, will live perhaps for weeks; and so, out of the chances from time to time occurring during long periods, reptiles will be the first to get safely landed on foreign shores: as indeed they are even now known sometimes to be. The transport of mammalia being comparatively precarious, must, in the order of probability, be longer postponed; and would, indeed, be unlikely to occur until by the enlargement of the new continent, the distances of its shores from adjacent lands had been greatly diminished, or the formation of intervening islands had increased the chances of survival. Assuming, however, that the facilities for immigration had become adequate; which would be the first mammals to arrive and live? Not large herbivores; for they would be soon drowned if by any accident carried out to sea. Not the carnivora; for these would lack appropriate food, even if they outlived the voyage. Small quadrupeds frequenting trees, and feeding on insects, would be those most likely both to be drifted away from their native lands and to find fit food in a new one. Insectivorous mammals, like in size to those found in the Trias and the Stonesfield slate, might naturally be looked for as the pioneers of the higher vertebrata. And if we suppose the facilities of communication to be again increased, either by a further shallowing of the intervening sea and a consequent multiplication of islands, or by an actual junction of the new continent with an old one, through continued upheavals;

we should finally have an influx of the larger and more perfect mammals.

Now rude as is this sketch of a process that would be extremely elaborate and involved, and open as some of its propositions are to criticisms which there is no space here to meet; no one will deny that it represents something like the biologic history of the supposed new continent. Details apart, it is manifest that simple organisms, able to flourish under simple conditions of life, would be the first successful immigrants; and that more complex organisms, needing for their existence the fulfilment of more complex conditions, would afterwards establish themselves in something like an ascending succession. At the one extreme we see every facility. The new individuals can be conveyed in the shape of minute germs; immense numbers of these are perpetually being carried in all directions to great distances by ocean-currents—either detached or attached to floating bodies; they can find nutriment wherever they arrive; and the resulting organisms can multiply asexually with great rapidity. At the other extreme, we see every difficulty. The new individuals must be conveyed in their adult forms; their numbers are, in comparison, utterly insignificant; they live on land, and are very unlikely to be carried out to sea; when so carried, the chances are immense against their escape from drowning, starvation, or death by cold; if they survive the transit, they must have a pre-existing Flora or Fauna to supply their special food; they require, also, the fulfilment of various other physical conditions; and unless at least two individuals of different sexes are safely landed, the race cannot be established. Manifestly, then, the immigration of each successively higher order of organisms, having, from one or other additional condition to be fulfilled, an enormously-increased probability against it, would naturally be separated from the immigration of a lower order by some period like a geologic epoch. And thus the successive sedimentary deposits formed while this new continent was undergoing gradual elevation, would seem to furnish clear evidence of a general progress in the forms of life. That lands thus raised up in the midst of a wide ocean, would first give origin to unfossiliferous strata; next, to strata containing only the lowest marine forms; next to strata containing only the higher marine forms, ascending finally to fish; and that the strata above these would contain reptiles, then small mammals, then great mammals; seems to us demonstrable. And if the succession of fossils presented by the strata of this supposed new continent, would thus simulate the succession presented by our own sedimentary series; must we not conclude that our own sedimentary series very possibly records nothing more than the phenomena accompanying one of these great upheavals? The probability of this conclusion being admitted, it must be admitted that the facts of Paleontology can never suffice either to prove or disprove the Development Hypothesis; but that the most they can do is to show whether the last few pages of the Earth's biologic history, are or are not in harmony with this hypothesis—whether the existing Flora and Fauna can or can not be affiliated upon the Flora and Fauna of the most recent geologic times.

# BAIN ON THE EMOTIONS AND THE WILL.

[*First published in* The Medico-Chirurgical Review *for January,* 1860.]

After the controversy between the Neptunists and the Vulcanists had been long carried on without definite results, there came a reaction against all speculative geology. Reasoning without adequate data having led to nothing, inquirers went into the opposite extreme, and confining themselves wholly to collecting data, relinquished reasoning. The Geological Society of London was formed with the express object of accumulating evidence; for many years hypotheses were forbidden at its meetings: and only of late have attempts to organize the mass of observations into consistent theory been tolerated.

This reaction and subsequent re-reaction, well illustrate the recent history of English thought in general. The time was when our countrymen speculated, certainly to as great an extent as any other people, on all those high questions which present themselves to the human intellect; and, indeed, a glance at the systems of philosophy that are or have been current on the Continent, suffices to show how much other nations owe to the discoveries of our ancestors. For a generation or two, however, these more abstract subjects have fallen into neglect; and, among those who plume themselves on being "practical," even into contempt. Partly, perhaps, a natural accompaniment of our rapid material growth, this intellectual phase has been in great measure due to the exhaustion of argument, and the necessity for better data. Not so much with a conscious recognition of the end to be subserved, as from an unconscious subordination to that rhythm traceable in social changes as in other things, an era of theorizing without observing, has been followed by an era of observing without theorizing. During this long-continued devotion to concrete science, an immense quantity of raw material for abstract science has been accumulated; and now there is obviously commencing a period in which this accumulated raw material will be organized into consistent theory. On all sides—equally in the inorganic sciences, in the science of life, and in the science of society—we may note the tendency to pass from the superficial and empirical to the more profound and rational.

In Psychology this change is conspicuous. The facts brought to light by anatomists and physiologists during the last fifty years, are at length being used towards the interpretation of this highest class of biological phenomena; and already there is promise of a great advance. The work of Mr. Alexander Bain, of which the second volume has been recently issued, may be regarded as especially characteristic of the transition. It gives us, in orderly arrangement, the great mass of evidence supplied by modern science towards the building-up of a coherent system of mental philosophy. It is not in itself a system of mental philosophy, properly so called; but a classified collection of materials for such a system, presented with that method and insight which scientific discipline generates, and accompanied with occasional passages of an analytical character. It is indeed that which it in the main professes to be—a natural history of the mind. Were we to say that the researches of the naturalist who collects and dissects and describes species, bear the same relation to the researches of the comparative anatomist tracing out the laws of organization, which Mr. Bain's labours bear to the labours of the abstract psychologist, we should be going somewhat too far; for Mr. Bain's work is not wholly descriptive. Still, however, such an analogy conveys the best general conception of what he has done; and serves most clearly to indicate its needfulness. For as, before there can be made anything like true generalizations respecting the classification of organisms and the laws of organization, there must be an extensive accumulation of the facts presented in numerous organic bodies; so, without a tolerably-complete delineation of mental phenomena of all orders, there can scarcely arise any adequate theory of mind. Until recently, mental science has been pursued much as physical science was pursued by the ancients; not by drawing conclusions from observations and experiments, but by drawing them from arbitrary *a priori* assumptions. This course, long since abandoned in the one case with immense advantage, is gradually being abandoned in the other; and the treatment of Psychology as a division of natural history, shows that the abandonment will soon be complete.

Estimated as a means to higher results, Mr. Bain's work is of great value. Of its kind it is the most scientific in conception, the most catholic in spirit, and the most complete in execution. Besides delineating the various classes of mental phenomena as seen under that stronger light thrown on them by modern science, it includes in the picture much which previous writers had omitted—partly from prejudice, partly from ignorance. We refer more especially to the participation of bodily organs in mental changes; and the addition to the primary mental changes, of those many secondary ones which the actions of the bodily organs generate. Mr. Bain has, we believe, been the first to appreciate the importance of this element in our states of consciousness; and it is one of his merits that he shows how constant and large an element it is. Further, the relations of voluntary and involuntary movements are elucidated in a way that was not possible to writers unacquainted with the modern doctrine of reflex action. And beyond this, some of the analytical passages that here and there occur, contain important ideas.

Valuable, however, as is Mr. Bain's work, we regard it as essentially transitional. It presents in a digested form the results of a period of observation; adds to these results many well-delineated facts collected by himself; arranges new and old materials with that more scientific method which the discipline of our times has fostered; and so prepares the way for better generalizations. But almost of necessity its classifications and conclusions are provisional. In the growth of each science, not only is correct observation needful for the formation of true theory; but true theory is needful as a preliminary to correct observation. Of course we do not intend this assertion to be taken literally; but as a strong expression of the fact that the two must advance hand in hand. The first crude theory or rough classification, based on very slight knowledge of the phenomena, is requisite as a means of reducing the phenomena to some kind of order; and as supplying a conception with which fresh phenomena may be compared, and their agreement or disagreement noted. Incongruities being by and by made manifest by wider examination of cases, there comes such modification of the theory as brings it into a nearer correspondence with the evidence. This reacts to the further advance of observation. More extensive and complete observation brings additional corrections of theory; and so on till the truth is reached. In mental science, the systematic collection of facts having but recently commenced, it is not to be expected that the results can be at once rightly formulated. All that may be looked for are approximate generalizations which will presently serve for the better directing of inquiry. Hence, even were it not now possible to say in what way it does so, we might be tolerably certain that Mr. Bain's work bears the stamp of the inchoate state of Psychology.

We think, however, that it will not be difficult to find in what respects its organization is provisional; and at the same time to show what must be the nature of a more complete organization. We propose here to attempt this: illustrating our positions from his recently-issued second volume.

Is it possible to make a true classification without the aid of analysis? or must there not be an analytical basis to every true classification? Can the real relations of things be determined by the obvious characteristics of the things? or does it not commonly happen that certain hidden characteristics, on which the obvious ones depend, are the truly significant ones? This is the preliminary question which a glance at Mr. Bain's scheme of the emotions suggests.

Though not avowedly, yet by implication, Mr. Bain assumes that a right conception of the nature, the order, and the relations of the emotions, may be arrived at by contemplating their conspicuous objective and subjective characters, as displayed in the adult. After pointing out that we lack those means of classification which serve in the case of the sensations, he says—

"In these circumstances we must turn our attention to *the manner of diffusion* of the different passions and emotions, in order to obtain a basis of classification analogous to the arrangement of the sensations. If what we have already advanced on that subject be at all well founded, this is the genuine turning point of the method to be chosen, for the same mode of diffusion will always be accompanied by the same mental experience, and each of the two aspects would identify, and would be evidence of, the other. There is, therefore, nothing so thoroughly characteristic of any state of feeling as the nature of the diffusive wave that embodies it, or the various organs specially roused into action by it, together with the manner of the action. The only drawback is our comparative ignorance, and our inability to discern the precise character of the diffusive currents in every case; a radical imperfection in the science of mind as constituted at present.

"Our own consciousness, formerly reckoned the only medium of knowledge to the mental philosopher, must therefore be still referred to as a principal means of discriminating the varieties of human feeling. We have the power of noting agreement and difference among our conscious states, and on this we can raise a structure of classification. We recognise such generalities as pleasure, pain, love, anger, through the property of mental or intellectual discrimination that accompanies in our mind the fact of emotion. A certain degree of precision is attainable by this mode of mental comparison and analysis; the farther we can carry such precision the better; but that is no reason why it should stand alone to the neglect of the corporeal embodiments through which one mind reveals itself to others. The companionship of inward feeling with bodily manifestation is a fact of the human constitution, and deserves to be studied as such; and it would be difficult to find a place more appropriate than a treatise on the mind for setting forth the conjunctions and sequences traceable in this department of nature. I shall make no scruple in conjoining with the description of the mental phenomena the physical appearances, in so far as I am able to ascertain them.

"There is still one other quarter to be referred to in settling a complete arrangement of the emotions, namely, the varieties of human conduct, and the machinery created in subservience to our common susceptibilities. For example, the vast superstructure of fine art has its foundations in human feeling, and in rendering an account of this we are led to recognise the interesting group of artistic or æsthetic emotions. The same outward reference to conduct and creations brings to light the so-called moral sense in man, whose foundations in the mental system have accordingly to be examined.

"Combining together these various indications, or sources of discrimination,—outward objects, diffusive mode or expression, inward consciousness, resulting conduct and institutions,—I adopt the following arrangement of the families or natural orders of emotion."

Here, then, are confessedly adopted, as bases of classification, the most manifest characters of the emotions; as discerned subjectively, and objectively. The mode of diffusion of an emotion is one of its outside aspects; the institutions it generates form another of its outside aspects; and though the peculiarities of the emotion as a state of consciousness, seem to express its intrinsic and ultimate nature, yet such peculiarities as are perceptible by simple introspection, must also be classed as superficial peculiarities. It is a familiar fact that various intellectual states of consciousness turn out, when analyzed, to have natures widely unlike those which at first appear; and we believe the like will prove true of emotional states of consciousness. Just as our concept of space, which is apt to be thought a simple, undecomposable concept, is yet resolvable into experiences quite different from that state of consciousness which we call space; so, probably, the sentiment of affection or reverence is compounded of elements that are severally distinct from the whole which they make up. And much as a classification of our ideas which dealt with the idea of space as though it were ultimate, would be a classification of ideas by their externals; so, a classification of our emotions, which, regarding them as simple, describes their aspects in ordinary consciousness, is a classification of emotions by their externals.

Thus, then, Mr. Bain's grouping is throughout determined by the most manifest attributes—those objectively displayed in the natural language of the emotions, and in the social phenomena that result from them, and those subjectively displayed in the aspects the emotions assume in an analytical consciousness. And the question is—Can they be correctly grouped after this method?

We think not; and had Mr. Bain carried farther an idea with which he has set out, he would probably have seen that they cannot. As already said, he avowedly adopts "the natural-history-method:" not only referring to it in his preface, but in his first chapter giving examples of botanical and zoological classifications, as illustrating the mode in which he proposes to deal with the emotions. This we conceive to be a philosophical conception; and we have only to regret that Mr. Bain has overlooked some of its most important implications. For in what has essentially consisted the progress of natural-history-classification? In the abandonment of grouping by external, conspicuous characters; and in the making of certain internal, but all-essential characters, the bases of groups. Whales are not now ranged along with fish, because in their general forms and habits of life they resemble fish; but they are ranged with mammals, because the type of their organization, as ascertained by dissection, corresponds with that of mammals. No longer considered as sea-weeds in virtue of their forms and modes of growth, *Polyzoa* are now shown, by examination of their economy, to belong to the animal kingdom. It is found, then, that the discovery of real relationships involves analysis. It has turned out that the earlier classifications, guided by general resemblances, though containing much truth, and though very useful provisionally, were yet in many cases radically wrong; and that the true affinities of organisms, and the true homologies of their parts, are to be made out only by examining their hidden structures. Another fact of great significance in the history of classification is also to be noted. Very frequently the kinship of an organism cannot be made out even by exhaustive analysis, if that analysis is confined to the adult structure. In many cases it is needful to examine the structure in its earlier stages; and even in its embryonic stage. So difficult was it, for instance, to determine the true position of the *Cirrhipedia* among animals, by examining mature individuals only, that Cuvier erroneously classed them with *Mollusca,* even after dissecting them; and not until their early forms were discovered, were they clearly proved to belong to the *Crustacea.* So important, indeed, is the study of development as a means to classification, that the first zoologists now hold it to be the only absolute criterion.

Here, then, in the advance of natural-history-classification, are two fundamental facts, which should be borne in mind when classifying the emotions. If, as Mr. Bain rightly assumes, the emotions are to be grouped after the natural-history-method; then it should be the natural-history-method in its complete form, and not in its rude form. Mr. Bain will doubtless agree in the belief, that a correct account of the emotions in their natures and relations, must correspond with a correct account of the nervous system—must form another side of the same ultimate facts. Structure and function must necessarily harmonize. Structures which have with each other certain ultimate connexions, must have functions which have answering connexions. Structures which have arisen in certain ways, must have functions which have arisen in parallel ways. And hence if analysis and development are needful for the right interpretation of structures, they must be needful for the right interpretation of functions. Just as a scientific description of the digestive organs must include not only their obvious forms and connexions, but their microscopic characters, and also the ways in which they severally result by differentiation from the primitive mucous membrane; so must a scientific account of the nervous system include its general arrangements, its minute structure, and its mode of evolution; and so must a scientific account of nervous actions include the answering three elements. Alike in classing separate organisms, and in classing the parts of the same organism, the complete natural-history-method involves ultimate analysis, aided by development; and Mr. Bain, in not basing his classification of the emotions on characters reached through these aids, has fallen short of the conception with which he set out.

"But," it will perhaps be asked, "how are the emotions to be analyzed, and their modes of evolution to be ascertained? Different animals, and different organs of the same animal, may readily be compared in their internal structures and microscopic structures, as also in their developments; but functions, and especially such functions as the emotions, do not admit of like comparisons."

It must be admitted that the application of these methods is here by no means so easy. Though we can note differences and similarities between the internal formations of two animals; it is difficult to contrast the mental states of two animals. Though the true morphological relations of organs may be made out by observation of embryos; yet, where such organs are inactive before birth, we cannot completely trace the history of their actions. Obviously, too, pursuance of inquiries of the kind indicated, raises questions which science is not yet prepared to answer; as, for instance—Whether all nervous functions, in common with all other functions, arise by gradual differentiations, as their organs do? Whether the emotions are, therefore, to be regarded as divergent modes of action that have become unlike by successive modifications? Whether, as two organs which originally budded out of the same membrane have not only become different as they developed, but have also severally become compound internally, though externally simple; so two emotions, simple and near akin in their roots, may not only have grown unlike, but may also have grown involved in their natures, though seeming homogeneous to consciousness? And here, indeed, in the inability of existing science to answer these questions which underlie a true psychological classification, we see how purely provisional any present classification is likely to be.

Nevertheless, even now, classification may be aided by development and ultimate analysis to a considerable extent; and the defect in Mr. Bain's work is, that he has not systematically availed himself of them as far as possible. Thus we may, in the first place, study the evolution of the emotions up through the various grades of the animal kingdom: observing which of them are earliest and exist with the lowest organization and intelligence; in what order the others accompany higher endowments; and how they are severally related to the conditions of life. In the second place, we may note the emotional differences between the lower and the higher human races—may regard as earlier and simpler those feelings which are common to both, and as later and more compound those which are characteristic of the most civilized. In the third place, we may observe the order in which the emotions unfold during the progress from infancy to maturity. And lastly, comparing these three kinds of emotional development, displayed in the ascending grades of the animal kingdom, in the advance of the civilized races, and in individual history, we may see in what respects they harmonize, and what are the implied general truths.

Having gathered together and generalized these several classes of facts, analysis of the emotions would be made easier. Setting out with the assumption that every new form of emotion making its appearance in the individual or the race, is a modification of some pre-existing emotion, or a compound of several pre-existing emotions, we should be greatly aided by knowing what always are the pre-existing emotions. When, for example, we find that very few of the lower animals show any love of accumulation, and that this feeling is absent in infancy — when we see that an infant in arms exhibits anger, fear, wonder, while yet it manifests no desire of permanent possession, and that a brute which has no acquisitiveness can nevertheless feel attachment, jealousy, love of approbation; we may suspect that the feeling which property satisfies is compounded out of simpler and deeper feelings. We may conclude that as, when a dog hides a bone, there must exist in him a prospective gratification of hunger; so there must similarly at first, in all cases where anything is secured or taken possession of, exist an ideal excitement of the feeling which that thing will gratify. We may further conclude that when the intelligence is such that a variety of objects come to be utilized for different purposes—when, as among savages, divers wants are satisfied through the articles appropriated for weapons, shelter, clothing, ornament; the act of appropriating comes to be one constantly involving agreeable associations, and one which is therefore pleasurable, irrespective of the end subserved. And when, as in civilized life, the property acquired is of a kind not conducing to one order of gratification in particular, but is capable of administering to all gratifications, the pleasure of acquiring property grows more distinct from each of the various pleasures subserved—is more completely differentiated into a separate emotion.

This illustration, roughly as it is sketched, will show what we mean by the use of comparative psychology in aid of classification. Ascertaining by induction the actual order of evolution of the emotions, we are led to suspect this to be their order of successive dependence; and are so led to recognize their order of ascending complexity; and by consequence their true groupings.

Thus, in the very process of arranging the emotions into grades, beginning with those involved in the lowest forms of conscious activity and ending with those peculiar to the adult civilized man, the way is opened for that ultimate analysis which alone can lead us to the true science of the matter. For when we find both that there exist in a man feelings which do not exist in a child, and that the European is characterized by some sentiments which are wholly or in great part absent from the savage—when we see that, besides the new emotions which arise spontaneously as the individual becomes completely organized, there are new emotions making their appearance in the more advanced divisions of our race; we are led to ask—How are new emotions generated? The lowest savages have not even the ideas of justice or mercy: they have neither words for them nor can they be made to conceive them; and the manifestation of them by Europeans they ascribe to fear or cunning. There are æsthetic emotions common among ourselves, which are scarcely in any degree experienced by some inferior races; as, for instance, those produced by music. To which instances may be added the less marked but more numerous contrasts that exist between civilized races in the degrees of their several emotions. And if it is manifest, both that all the emotions are capable of being permanently modified in the course of successive generations, and that what must be classed as new emotions may be brought into existence; then it follows that nothing like a true conception of the emotions is to be obtained, until we understand how they are evolved.

Comparative Psychology, while it raises this inquiry, prepares the way for answering it. When observing the differences between races, we can scarcely fail to observe also how these differences correspond with differences between their conditions of existence, and consequent activities. Among the lowest races of men, love of property stimulates to the obtainment only of such things as satisfy immediate desires, or desires of the immediate future. Improvidence is the rule: there is little effort to meet remote contingencies. But the growth of established societies having gradually given security of possession, there has been an increasing tendency to provide for coming years: there has been a constant exercise of the feeling which is satisfied by a provision for the future; and there has been a growth of this feeling so great that it now prompts accumulation to an extent beyond what is needful. Note, again, that under the discipline of social life—under a comparative abstinence from aggressive actions, and a performance of those naturally-serviceable actions implied by the division of labour—there has been a development of those gentle emotions of which inferior races exhibit but the rudiments. Savages delight in giving pain rather than pleasure—are almost devoid of sympathy; while among ourselves, philanthropy organizes itself in laws, establishes numerous institutions, and dictates countless private benefactions.

From which and other like facts, does it not seem an unavoidable inference, that new emotions are developed by new experiences—new habits of life? All are familiar with the truth that, in the individual, each feeling may be strengthened by performing those actions which it prompts; and to say that the feeling is *strengthened,* is to say that it is in part *made* by these actions. We know, further, that not unfrequently, individuals, by persistence in special courses of conduct, acquire special likings for such courses, disagreeable as these may be to others; and these whims, or morbid tastes, imply incipient emotions corresponding to these special activities. We know that emotional characteristics, in common with all others, are hereditary; and the differences between civilized nations descended from the same stock, show us the cumulative results of small modifications hereditarily transmitted. And when we see that between savage and civilized races which diverged from one another in the remote past, and have for a hundred generations followed modes of life becoming ever more unlike, there exist still greater emotional contrasts; may we not infer that the more or less distinct emotions which characterize civilized races, are the organized results of certain daily-repeated combinations of mental states which social life involves? Must we not say that habits not only modify emotions in the individual, and not only beget tendencies to like habits and accompanying emotions in descendants, but that when the conditions of the race make the habits persistent, this progressive modification may go on to the extent of producing emotions so far distinct as to seem new? And if so, we may suspect that such new emotions, and by implication all emotions analytically considered, consist of aggregated and consolidated groups of those simpler feelings which habitually occur together in experience. When, in the circumstances of any race, some one kind of action or set of actions, sensation or set of sensations, is usually followed, or accompanied, by various other sets of actions or sensations, and so entails a large mass of pleasurable or painful states of consciousness; these, by frequent repetition, become so connected together that the initial action or sensation brings the ideas of all the rest crowding into consciousness: producing, in some degree, the pleasures or pains that have before been felt in reality. And when this relation, besides being frequently repeated in the individual, occurs in successive generations, all the many nervous actions involved tend to grow organically connected. They become incipiently reflex; and, on the occurrence of the appropriate stimulus, the whole nervous apparatus which in past generations was brought into activity by this stimulus, becomes nascently excited. Even while yet there have been no individual experiences, a vague feeling of pleasure or pain is produced; constituting what we may call the body of the emotion. And when the experiences of past generations come to be repeated in the individual, the emotion gains both strength and definiteness; and is accompanied by the appropriate specific ideas.

This view of the matter, which we believe the established truths of Physiology and Psychology unite in indicating, and which is the view that generalizes the phenomena of habit, of national characteristics, of civilization in its moral aspects, at the same time that it gives us a conception of emotion in its origin and ultimate nature, may be illustrated from the mental modifications undergone by animals. On newly-discovered lands not inhabited by man, birds are so devoid of fear as to allow themselves to be knocked over with sticks; but in the course of generations, they acquire such a dread of man as to fly on his approach; and this dread is manifested by young as well as by old. Now unless this change be ascribed to the killing-off of the less fearful, and the preservation and multiplication of the more fearful, which, considering the comparatively small number killed by man, is an inadequate cause; it must be ascribed to accumulated experiences; and each experience must be held to have a share in producing it. We must conclude that in each bird which escapes with injuries inflicted by man, or is alarmed by the outcries of other members of the flock (gregarious creatures of any intelligence being necessarily more or less sympathetic), there is established an association of ideas between the human aspect and the pains, direct and indirect, suffered from human agency. And we must further conclude that the state of consciousness which impels the bird to take flight, is at first nothing more than an ideal reproduction of those painful impressions which before followed man's approach; that such ideal reproduction becomes more vivid and more massive as the painful experiences, direct or sympathetic, increase; and that thus the emotion in its incipient state, is nothing else than an aggregation of the revived pains before experienced. As, in the course of generations, the young birds of this race begin to display a fear of man before yet they have been injured by him, it is an unavoidable inference that the nervous system of the race has been organically modified by these experiences: we have no choice but to conclude that when a young bird is thus led to fly, it is because the impression produced on its senses by the approaching man, entails, through an incipiently-reflex action, a partial excitement of all those nerves which in its ancestors had been excited under the like conditions; that this partial excitement has its accompanying painful consciousness; and that the vague painful consciousness thus arising, constitutes emotion proper—*emotion undecomposable into specific experiences, and therefore seemingly homogeneous.*

If such be the explanation of the fact in this case, then it is in all cases. If emotion is so generated here, then it is so generated throughout. We must perforce conclude that the emotional modifications displayed by different nations, and those higher emotions by which civilized are distinguished from savage, are to be accounted for on the same principle. And concluding this, we are led strongly to suspect that the emotions in general have severally thus originated.

Perhaps we have now made sufficiently clear what we mean by the study of the emotions through analysis and development. We have aimed to justify the positions that, without analysis aided by development, there cannot be a true natural history of the emotions; and that a natural history of the emotions based on external characters can be but provisional. We think that Mr. Bain, in confining himself to an account of the emotions as they exist in the adult civilized man, has neglected those classes of facts out of which the science of the matter must chiefly be built. It is true that he has treated of habits as modifying emotions in the individual; but he has not recognized the fact that where conditions render habits persistent in successive generations, such modifications are cumulative: he has not hinted that the modifications produced by habit are emotions in the making. It is true, also, that he occasionally refers to the characteristics of children; but he does not systematically trace the changes through which childhood passes into manhood, as throwing light on the order and genesis of the emotions. It is further true that he here and there refers to national traits in illustration of his subject; but these stand as isolated facts, having no general significance: there is no hint of any relation between them and the national circumstances; while all those many moral contrasts between lower and higher races which throw great light on classification, are passed over. And once more, it is true that many passages of his work, and sometimes, indeed, whole sections of it, are analytical; but his analyses are incidental—they do not underlie his entire scheme, but are here and there added to it. In brief, he has written a Descriptive Psychology, which does not appeal to Comparative Psychology and Analytical Psychology for its leading ideas. And in doing this, he has omitted much that should be included in a natural history of the mind; while to that part of the subject with which he has dealt, he has given a necessarily-imperfect organization.

Even leaving out of view the absence of those methods and criteria on which we have been insisting, it appears to us that meritorious as is Mr. Bain's book in its details, it is defective in some of its leading ideas. The first paragraphs of his first chapter, quite startled us by the strangeness of their definitions—a strangeness which can scarcely be ascribed to laxity of expression. The paragraphs run thus:—

> "Mind is comprised under three heads,—Emotion, Volition, and Intellect.
>
> "EMOTION is the name here used to comprehend all that is understood by feelings, states of feeling, pleasures, pains, passions, sentiments, affections. Consciousness, and conscious states also for the most part denote modes of emotion, although there is such a thing as the Intellectual consciousness.
>
> "VOLITION, on the other hand, indicates the great fact that our Pleasures and Pains, which are not the whole of our emotions, prompt to action, or stimulate the active machinery of the living framework to perform such operations as procure the first and abate the last. To withdraw from a scalding heat, and cling to a gentle warmth, are exercises of volition."

The last of these definitions, which we may most conveniently take first, seems to us very faulty. We cannot but feel astonished that Mr. Bain, familiar as he is with the phenomena of reflex action, should have so expressed himself as to include a great part of them along with the phenomena of volition. He seems to be ignoring the discriminations of modern science, and returning to the vague conceptions of the past—nay more, he is comprehending under volition what even the popular speech would hardly bring under it. If you were to blame any one for snatching his foot from the scalding water into which he had inadvertently put it, he would tell you that he could not help it; and his reply would be indorsed by the general experience, that the withdrawal of a limb from contact with something extremely hot, is quite involuntary—that it takes place not only without volition, but in defiance of an effort of will to maintain the contact. How, then, can that be instanced as an example of volition, which occurs even when volition is antagonistic? We are quite aware that it is impossible to draw any absolute line of demarcation between automatic actions and actions which are not automatic. Doubtless we may pass gradually from the purely reflex, through the consensual, to the voluntary. Taking the case Mr. Bain cites, it is manifest that from a heat of such moderate degree that the withdrawal from it is wholly voluntary, we may advance by infinitesimal steps to a heat which compels involuntary withdrawal; and that there is a stage at which the voluntary and involuntary actions are mixed. But the difficulty of absolute discrimination is no reason for neglecting the broad general contrast; any more than it is for confounding light with darkness. If we are to include as examples of volition, all cases in which pleasures and pains "stimulate the active machinery of the living framework to perform such operations as procure the first and abate the last," then we must consider sneezing and coughing as examples of volition; and Mr. Bain surely cannot mean this. Indeed, we must confess ourselves at a loss. On the one hand if he does not mean it, his expression is lax to a degree that surprises us in so careful a writer. On the other hand, if he does mean it, we cannot understand his point of view.

A parallel criticism applies to his definition of Emotion. Here, too, he has departed from the ordinary acceptation of the word; and, as we think, in the wrong direction. Whatever may be the interpretation that is justified by its derivation, the word emotion has come generally to mean that kind of feeling which is not a direct result of any action on the organism; but is either an indirect result of such action, or arises quite apart from such action. It is used to indicate those sentient states which are independently generated in consciousness; as distinguished from those generated in our corporeal framework, and known as sensations. Now this distinction, tacitly made in common speech, is one which Psychology cannot well reject; but one which it must adopt, and to which it must give scientific precision. Mr. Bain, however, appears to ignore any such distinction. Under the term emotion, he includes not only passions, sentiments, affections, but all "feelings, states of feeling, pleasures, pains,"—that is, all sensations. This does not appear to be a mere lapse of expression; for when, in the opening sentence, he asserts that "mind is comprised under the three heads—Emotion, Volition, and Intellect," he of necessity implies that sensation is included under one of these heads; and as it cannot be included under volition or intellect, it must be classed with emotion; as it clearly is in the next sentence.

We cannot but think this a retrograde step. Though distinctions which have been established in popular thought and language, are not unfrequently merged in the higher generalizations of science (as, for instance, when crabs and worms are grouped together in the sub-kingdom *Annulosa*); yet science very generally recognizes the validity of these distinctions, as real though not fundamental. And so in the present case. Such community as analysis discloses between sensation and emotion, must not shut out the broad contrast that exists between them. If there needs a wider word, as there does, to signify any sentient state whatever; then we may fitly adopt for this purpose the word currently so used, namely, "Feeling." And considering as Feelings all that great division of mental states which we do not class as Cognitions, we may then separate this great division into the two orders, Sensations and Emotions.

And here we may, before concluding, briefly indicate the leading outlines of a classification which reduces this distinction to a scientific form, and develops it somewhat further—a classification which, while suggested by certain fundamental traits reached without a very lengthened inquiry, is yet, we believe, in harmony with that disclosed by detailed analysis.

Leaving out of view the Will, which is a simple homogeneous mental state, forming the link between feeling and action, and not admitting of subdivisions; our states of consciousness fall into two great classes—COGNITIONS and FEELINGS.

COGNITIONS, or those modes of mind in which we are occupied with the *relations* that subsist among our feelings, are divisible into four great sub-classes.

*Presentative cognitions;* or those in which consciousness is occupied in localizing a sensation impressed on the organism—occupied, that is, with the relation between this presented mental state and those other presented mental states which make up our consciousness of the part affected: as when we cut ourselves.

*Presentative-representative cognitions;* or those in which consciousness is occupied with the relation between a sensation or group of sensations and the representations of those various other sensations that accompany it in experience. This is what we commonly call perception—an act in which, along with certain impressions presented to consciousness, there arise in consciousness the ideas of certain other impressions ordinarily connected with the presented ones: as when its visible form and colour, lead us to mentally endow an orange with all its other attributes.

*Representative cognitions;* or those in which consciousness is occupied with the relations among ideas or represented sensations; as in all acts of recollection.

*Re-representative cognitions;* or those in which the occupation of consciousness is not by representation of special relations that have before been presented to consciousness; but those in which such represented special relations are thought of merely as comprehended in a general relation—those in which the concrete relations once experienced, in so far as they become objects of consciousness at all, are incidentally represented, along with the abstract relation which formulates them. The ideas resulting from this abstraction, do not themselves represent actual experiences; but are symbols which stand for groups of such actual experiences—represent aggregates of representations. And thus they may be called re-representative cognitions. It is clear that the process of re-representation is carried to higher stages, as the thought becomes more abstract.

FEELINGS, or those modes of mind in which we are occupied, not with the relations subsisting between our sentient states, but with the sentient states themselves, are divisible into four parallel sub-classes.

*Presentative feelings,* ordinarily called sensations, are those mental states in which, instead of regarding a corporeal impression as of this or that kind, or as located here or there, we contemplate it in itself as pleasure or pain; as when eating.

*Presentative-representative feelings,* embracing a great part of what we commonly call emotions, are those in which a sensation, or group of sensations, or group of sensations and ideas, arouses a vast aggregation of represented sensations; partly of individual experience, but chiefly deeper than individual experience, and, consequently, indefinite. The emotion of terror may serve as an example. Along with certain impressions made on the eyes or ears, or both, are recalled in consciousness many of the pains to which such impressions have before been the antecedents; and when the relation between such impressions and such pains has been habitual in the race, the definite ideas of such pains which individual experience has given, are accompanied by the indefinite pains that result from inherited effects of experiences—vague feelings which we may call organic representations. In an infant, crying at a strange sight or sound while yet in the nurse's arms, we see these organic representations called into existence in the shape of dim discomfort, to which individual experience has yet given no specific outlines.

*Representative feelings,* comprehending the ideas of the feelings above classed, when they are called up apart from the appropriate external excitements. As instances of these may be named the feelings with which the descriptive poet writes, and which are aroused in the minds of his readers.

*Re-representative feelings,* under which head are included those more complex sentient states that are less the direct results of external excitements than the indirect or reflex results of them. The love of property is a feeling of this kind. It is awakened not by the presence of any special object, but by ownable objects at large; and it is not from the mere presence of such object, but from a certain ideal relation to them, that it arises. As before shown (p. 253) it consists, not of the represented advantages of possessing this or that, but of the represented advantages of possession in general—is not made up of certain concrete representations, but of the abstracts of many concrete representations; and so is re-representative. The higher sentiments, as that of justice, are still more completely of this nature. Here the sentient state is compounded out of sentient states that are themselves wholly, or almost wholly, re-representative: it involves representations of those lower emotions which are produced by the possession of property, by freedom of action, etc.; and thus is re-representative in a higher degree.

This classification, here roughly indicated and capable of further expansion, will be found in harmony with the results of detailed analysis aided by development. Whether we trace mental progression through the grades of the animal kingdom, through the grades of mankind, or through the stages of individual growth; it is obvious that the advance, alike in cognitions and feelings, is, and must be, from the presentative to the more and more remotely representative. It is undeniable that intelligence ascends from those simple perceptions in which consciousness is occupied in localizing and classifying sensations, to perceptions more and more compound, to simple reasoning, to reasoning more and more complex and abstract—more and more remote from sensation. And in the evolution of feelings, there is a parallel series of steps. Simple sensations; sensations combined together; sensations combined with represented sensations; represented sensations organized into groups, in which their separate characters are very much merged; representations of these representative groups, in which the original components have become still more vague. In both cases, the progress has necessarily been from the simple and concrete to the complex and abstract; and as with the cognitions, so with the feelings, this must be the basis of classification.

The space here occupied with criticisms on Mr. Bain's work, we might have filled with exposition and eulogy, had we thought this the more important. Though we have freely pointed out what we conceive to be its defects, let it not be inferred that we question its great merits. We repeat that, as a natural history of the mind, we believe it to be the best yet produced. It is a most valuable collection of carefully-elaborated materials. Perhaps we cannot better express our sense of its worth, than by saying that, to those who hereafter give to this branch of Psychology a thoroughly scientific organization, Mr. Bain's book will be indispensable.

# THE SOCIAL ORGANISM.

[*First published in* The Westminster Review *for January*, 1860.]

SIR JAMES MACINTOSH got great credit for the saying, that "constitutions are not made, but grow." In our day, the most significant thing about this saying is, that it was ever thought so significant. As from the surprise displayed by a man at some familiar fact, you may judge of his general culture; so from the admiration which an age accords to a new thought, its average degree of enlightenment may be inferred. That this apophthegm of Macintosh should have been quoted and requoted as it has, shows how profound has been the ignorance of social science. A small ray of truth has seemed brilliant, as a distant rushlight looks like a star in the surrounding darkness.

Such a conception could not, indeed, fail to be startling when let fall in the midst of a system of thought to which it was utterly alien. Universally in Macintosh's day, things were explained on the hypothesis of manufacture, rather than that of growth; as indeed they are, by the majority, in our own day. It was held that the planets were severally projected round the Sun from the Creator's hand, with just the velocity required to balance the Sun's attraction. The formation of the Earth, the separation of sea from land, the production of animals, were mechanical works from which God rested as a labourer rests. Man was supposed to be moulded after a manner somewhat akin to that in which a modeller makes a clay-figure. And of course, in harmony with such ideas, societies were tacitly assumed to be arranged thus or thus by direct interposition of Providence; or by the regulations of law-makers; or by both.

Yet that societies are not artificially put together, is a truth so manifest, that it seems wonderful men should ever have overlooked it. Perhaps nothing more clearly shows the small value of historical studies, as they have been commonly pursued. You need but to look at the changes going on around, or observe social organization in its leading traits, to see that these are neither supernatural, nor are determined by the wills of individual men, as by implication the older historians teach; but are consequent on general natural causes. The one case of the division of labour suffices to prove this. It has not been by command of any ruler that some men have become manufacturers, while others have remained cultivators of the soil. In Lancashire, millions have devoted themselves to the making of cotton-fabrics; in Yorkshire, another million lives by producing woollens; and the pottery of Staffordshire, the cutlery of Sheffield, the hardware of Birmingham, severally occupy their hundreds of thousands. These are large facts in the structure of English society; but we can ascribe them neither to miracle, nor to legislation. It is not by "the hero as king," any more than by "collective wisdom," that men have been segregated into producers, wholesale distributors, and retail distributors. Our industrial organization, from its main outlines down to its minutest details, has become what it is, not simply without legislative guidance, but, to a considerable extent, in spite of legislative hindrances. It has arisen under the pressure of human wants and resulting activities. While each citizen has been pursuing his individual welfare, and none taking thought about division of labour, or conscious of the need of it, division of labour has yet been ever becoming more complete. It has been doing this slowly and silently: few having observed it until quite modern times. By steps so small, that year after year the industrial arrangements have seemed just what they were before—by changes as insensible as those through which a seed passes into a tree; society has become the complex body of mutually-dependent workers which we now see. And this economic organization, mark, is the all-essential organization. Through the combination thus spontaneously evolved, every citizen is supplied with daily necessaries; while he yields some product or aid to others. That we are severally alive to-day, we owe to the regular working of this combination during the past week; and could it be suddenly abolished, multitudes would be dead before another week ended. If these most conspicuous and vital arrangements of our social structure have arisen not by the devising of any one, but through the individual efforts of citizens to satisfy their own wants; we may be tolerably certain that the less important arrangements have similarly arisen.

"But surely," it will be said, "the social changes directly produced by law, cannot be classed as spontaneous growths. When parliaments or kings order this or that thing to be done, and appoint officials to do it, the process is clearly artificial; and society to this extent becomes a manufacture rather than a growth." No, not even these changes are exceptions, if they be real and permanent changes. The true sources of such changes lie deeper than the acts of legislators. To take first the simplest instance. We all know that the enactments of representative governments ultimately depend on the national will: they may for a time be out of harmony with it, but eventually they must conform to it. And to say that the national will finally determines them, is to say that they result from the average of individual desires; or, in other words—from the average of individual natures. A law so initiated, therefore, really grows out of the popular character. In the case of a Government representing a dominant class, the same thing holds, though not so manifestly. For the very existence of a class monopolizing all power, is due to certain sentiments in the commonalty. Without the feeling of loyalty on the part of retainers, a feudal system could not exist. We see in the protest of the Highlanders against the abolition of heritable jurisdictions, that they preferred that kind of local rule. And if to the popular nature must be ascribed the growth of an irresponsible ruling class; then to the popular nature must be ascribed the social arrangements which that class creates in the pursuit of its own ends. Even where the Government is despotic, the doctrine still holds. The character of the people is, as before, the original source of this political form; and, as we have abundant proof, other forms suddenly created will not act, but rapidly retrograde to the old form. Moreover, such regulations as a despot makes, if really operative, are so because of their fitness to the social state. His acts being very much swayed by general opinion—by precedent, by the feeling of his nobles, his priesthood, his army—are in part immediate results of the national character; and when they are out of harmony with the national character, they are soon practically abrogated. The failure of Cromwell permanently to establish a new social condition, and the rapid revival of suppressed institutions and practices after his death, show how powerless is a monarch to change the type of the society he governs. He may disturb, he may retard, or he may aid the natural process of organization; but the general course of this process is beyond his control. Nay, more than this is true. Those who regard the histories of societies as the histories of their great men, and think that these great men shape the fates of their societies, overlook the truth that such great men are the products of their societies. Without certain antecedents—without a certain average national character, they neither could have been generated nor could have had the culture which formed them. If their society is to some extent re-moulded by them, they were, both before and after birth, moulded by their society—were the results of all those influences which fostered the ancestral character they inherited, and gave their own early bias, their creed, morals, knowledge, aspirations. So that such social changes as are immediately traceable to individuals of unusual power, are still remotely traceable to the social causes which produced these individuals; and hence, from the highest point of view, such social changes also, are parts of the general developmental process.

Thus that which is so obviously true of the industrial structure of society, is true of its whole structure. The fact that "constitutions are not made, but grow," is simply a fragment of the much larger fact, that under all its aspects and through all its ramifications, society is a growth and not a manufacture.

A perception that there exists some analogy between the body politic and a living individual body, was early reached; and has from time to time re-appeared in literature. But this perception was necessarily vague and more or less fanciful. In the absence of physiological science, and especially of those comprehensive generalizations which it has but lately reached, it was impossible to discern the real parallelisms.

The central idea of Plato's model Republic, is the correspondence between the parts of a society and the faculties of the human mind. Classifying these faculties under the heads of Reason, Will, and Passion, he classifies the members of his ideal society under what he regards as three analogous heads:—councillors, who are to exercise government; military or executive, who are to fulfil their behests; and the commonalty, bent on gain and selfish gratification. In other words, the ruler, the warrior, and the craftsman, are, according to him, the analogues of our reflective, volitional, and emotional powers. Now even were there truth in the implied assumption of a parallelism between the structure of a society and that of a man, this classification would be indefensible. It might more truly be contended that, as the military power obeys the commands of the Government, it is the Government which answers to the Will; while the military power is simply an agency set in motion by it. Or, again, it might be contended that whereas the Will is a product of predominant desires, to which the Reason serves merely as an eye, it is the craftsmen, who, according to the alleged analogy, ought to be the moving power of the warriors.

Hobbes sought to establish a still more definite parallelism: not, however, between a society and the human mind, but between a society and the human body. In the introduction to the work in which he develops this conception, he says—

> "For by art is created that great LEVIATHAN called a COMMONWEALTH, or STATE, in Latin CIVITAS, which is but an artificial man; though of greater stature and strength than the natural, for whose protection and defence it was intended, and in which the *sovereignty* is an artificial *soul*, as giving life and motion to the whole body; the *magistrates* and other *officers* of judicature and execution, artificial *joints; reward* and *punishment*, by which, fastened to the seat of the sovereignty, every joint and member is moved to perform his duty, are the *nerves*, that do the same in the body natural; the *wealth* and *riches* of all the particular members are the *strength; salus populi*, the *people's safety*, its *business; counsellors*, by whom all things needful for it to know are suggested unto it, are the *memory; equity* and *laws* an artificial *reason* and *will; concord, health; sedition, sickness;* and *civil war, death.*"

And Hobbes carries this comparison so far as actually to give a drawing of the Leviathan—a vast human-shaped figure, whose body and limbs are made up of multitudes of men. Just noting that these different analogies asserted by Plato and Hobbes, serve to cancel each other (being, as they are, so completely at variance), we may say that on the whole those of Hobbes are the more plausible. But they are full of inconsistencies. If the sovereignty is the soul of the body-politic, how can it be that magistrates, who are a kind of deputy-sovereigns, should be comparable to *joints?* Or, again, how can the three mental functions, memory, reason, and will, be severally analogous, the first to counsellors, who are a class of public officers, and the other two to equity and laws, which are not classes of officers, but abstractions? Or, once more, if magistrates are the artificial joints of society, how can reward and punishment be its nerves? Its nerves must surely be some class of persons. Reward and punishment must in societies, as in individuals, be *conditions* of the nerves, and not the nerves themselves.

But the chief errors of these comparisons made by Plato and Hobbes, lie much deeper. Both thinkers assume that the organization of a society is comparable, not simply to the organization of a living body in general, but to the organization of the human body in particular. There is no warrant whatever for assuming this. It is in no way implied by the evidence; and is simply one of those fancies which we commonly find mixed up with the truths of early speculation. Still more erroneous are the two conceptions in this, that they construe a society as an artificial structure. Plato's model republic—his ideal of a healthful body-politic—is to be consciously put together by men, just as a watch might be; and Plato manifestly thinks of societies in general as thus originated. Quite specifically does Hobbes express a like view. "For by *art*," he says, "is created that great LEVIATHAN called a COMMONWEALTH." And he even goes so far as to compare the supposed social contract, from which a society suddenly originates, to the creation of a man by the divine fiat. Thus they both fall into the extreme inconsistency of considering a community as similar in structure to a human being, and yet as produced in the same way as an artificial mechanism—in nature, an organism; in history, a machine.

Notwithstanding errors, however, these speculations have considerable significance. That such likenesses, crudely as they are thought out, should have been alleged by Plato and Hobbes and others, is a reason for suspecting that *some* analogy exists. The untenableness of the particular parallelisms above instanced, is no ground for denying an essential parallelism; since early ideas are usually but vague adumbrations of the truth. Lacking the great generalizations of biology, it was, as we have said, impossible to trace out the real relations of social organizations to organizations of another order. We propose here to show what are the analogies which modern science discloses.

Let us set out by succinctly stating the points of similarity and the points of difference. Societies agree with individual organisms in four conspicuous peculiarities:—

1. That commencing as small aggregations, they insensibly augment in mass: some of them eventually reaching ten thousand times what they originally were.

2. That while at first so simple in structure as to be considered structureless, they assume, in the course of their growth, a continually-increasing complexity of structure.

3. That though in their early, undeveloped states, there exists in them scarcely any mutual dependence of parts, their parts gradually acquire a mutual dependence; which becomes at last so great, that the activity and life of each part is made possible only by the activity and life of the rest.

4. That the life of a society is independent of, and far more prolonged than, the lives of any of its component units; who are severally born, grow, work, reproduce, and die, while the body-politic composed of them survives generation after generation, increasing in mass, in completeness of structure, and in functional activity.

These four parallelisms will appear the more significant the more we contemplate them. While the points specified, are points in which societies agree with individual organisms, they are also points in which individual organisms agree with one another, and disagree with all things else. In the course of its existence, every plant and animal increases in mass, in a way not paralleled by inorganic objects: even such inorganic objects as crystals, which arise by growth, show us no such definite relation between growth and existence as organisms do. The orderly progress from simplicity to complexity, displayed by bodies-politic in common with living bodies, is a characteristic which distinguishes living bodies from the inanimate bodies amid which they move. That functional dependence of parts, which is scarcely more manifest in animals than in nations, has no counterpart elsewhere. And in no aggregate except an organic or a social one, is there a perpetual removal and replacement of parts, joined with a continued integrity of the whole. Moreover, societies and organisms are not only alike in these peculiarities, in which they are unlike all other things; but the highest societies, like the highest organisms, exhibit them in the greatest degree. We see that the lowest animals do not increase to anything like the sizes of the higher ones; and, similarly, we see that aboriginal societies are comparatively limited in their growths. In complexity, our large civilized nations as much exceed primitive savage tribes, as a mammal does a zoophyte. Simple communities, like simple creatures, have so little mutual dependence of parts, that mutilation or subdivision causes but little inconvenience; but from complex communities, as from complex creatures, you cannot remove any considerable organ without producing great disturbance or death of the rest. And in societies of low type, as in inferior animals, the life of the aggregate, often cut short by division or dissolution, exceeds in length the lives of the component units, very far less than in civilized communities and superior animals; which outlive many generations of their component units.

On the other hand, the leading differences between societies and individual organisms are these:—

1. That societies have no specific external forms. This, however, is a point of contrast which loses much of its

importance, when we remember that throughout the vegetal kingdom, as well as in some lower divisions of the animal kingdom, the forms are often very indefinite—definiteness being rather the exception than the rule; and that they are manifestly in part determined by surrounding physical circumstances, as the forms of societies are. If, too, it should eventually be shown, as we believe it will, that the form of every species of organism has resulted from the average play of the external forces to which it has been subject during its evolution as a species; then, that the external forms of societies should depend, as they do, on surrounding conditions, will be a further point of community.

2. That though the living tissue whereof an individual organism consists, forms a continuous mass, the living elements of a society do not form a continuous mass; but are more or less widely dispersed over some portion of the Earth's surface. This, which at first sight appears to be an absolute distinction, is one which yet to a great extent fades when we contemplate all the facts. For, in the lower divisions of the animal and vegetal kingdoms, there are types of organization much more nearly allied, in this respect, to the organization of a society, than might be supposed—types in which the living units essentially composing the mass, are dispersed through an inert substance, that can scarcely be called living in the full sense of the word. It is thus with some of the *Protococci* and with the *Nostoceæ*, which exist as cells imbedded in a viscid matter. It is so, too, with the *Thalassicollæ*—bodies made up of differentiated parts, dispersed through an undifferentiated jelly. And throughout considerable portions of their bodies, some of the *Acalephæ* exhibit more or less this type of structure. Now this is very much the case with a society. For we must remember that though the men who make up a society are physically separate, and even scattered, yet the surface over which they are scattered is not one devoid of life, but is covered by life of a lower order which

ministers to their life. The vegetation which clothes a country makes possible the animal life in that country; and only through its animal and vegetal products can such a country support a society. Hence the members of the body-politic are not to be regarded as separated by intervals of dead space, but as diffused through a space occupied by life of a lower order. In our conception of a social organism, we must include all that lower organic existence on which human existence, and therefore social existence, depend. And when we do this, we see that the citizens who make up a community may be considered as highly vitalized units surrounded by substances of lower vitality, from which they draw their nutriment: much as in the cases above instanced.

3. The third difference is that while the ultimate living elements of an individual organism are mostly fixed in their relative positions, those of the social organism are capable of moving from place to place. But here, too, the disagreement is much less than would be supposed. For while citizens are locomotive in their private capacities, they are fixed in their public capacities. As farmers, manufacturers, or traders, men carry on their businesses at the same spots, often throughout their whole lives; and if they go away occasionally, they leave behind others to discharge their functions in their absence. Each great centre of production, each manufacturing town or district, continues always in the same place; and many of the firms in such town or district, are for generations carried on either by the descendants or successors of those who founded them. Just as in a living body, the cells that make up some important organ severally perform their functions for a time and then disappear, leaving others to supply their places; so, in each part of a society the organ remains, though the persons who compose it change. Thus, in social life, as in the life of an animal, the units as well as the larger agencies formed of them, are in the main

stationary as respects the places where they discharge their duties and obtain their sustenance. And hence the power of individual locomotion does not practically affect the analogy.

4. The last and perhaps the most important distinction is, that while in the body of an animal only a special tissue is endowed with feeling, in a society all the members are endowed with feeling. Even this distinction, however, is not a complete one. For in some of the lowest animals, characterized by the absence of a nervous system, such sensitiveness as exists is possessed by all parts. It is only in the more organized forms that feeling is monopolized by one class of the vital elements. And we must remember that societies, too, are not without a certain differentiation of this kind. Though the units of a community are all sensitive, they are so in unequal degrees. The classes engaged in laborious occupations are less susceptible, intellectually and emotionally, than the rest; and especially less so than the classes of highest mental culture. Still, we have here a tolerably decided contrast between bodies-politic and individual bodies; and it is one which we should keep constantly in view. For it reminds us that while, in individual bodies, the welfare of all other parts is rightly subservient to the welfare of the nervous system, whose pleasurable or painful activities make up the good or ill of life; in bodies-politic the same thing does not hold, or holds to but a very slight extent. It is well that the lives of all parts of an animal should be merged in the life of the whole, because the whole has a corporate consciousness capable of happiness or misery. But it is not so with a society; since its living units do not and cannot lose individual consciousness, and since the community as a whole has no corporate consciousness. This is an everlasting reason why the welfares of citizens cannot rightly be sacrificed to some supposed benefit of the State, and why, on the other hand, the State is to be maintained solely

for the benefit of citizens. The corporate life must here be subservient to the lives of the parts, instead of the lives of the parts being subservient to the corporate life.

Such, then, are the points of analogy and the points of difference. May we not say that the points of difference serve but to bring into clearer light the points of analogy? While comparison makes definite the obvious contrasts between organisms commonly so called, and the social organism, it shows that even these contrasts are not so decided as was to be expected. The indefiniteness of form, the discontinuity of the parts, and the universal sensitiveness, are not only peculiarities of the social organism which have to be stated with considerable qualifications; but they are peculiarities to which the inferior classes of animals present approximations. Thus we find but little to conflict with the all-important analogies. Societies slowly augment in mass; they progress in complexity of structure; at the same time their parts become more mutually dependent; their living units are removed and replaced without destroying their integrity; and the extents to which they display these peculiarities are proportionate to their vital activities. These are traits that societies have in common with organic bodies. And these traits in which they agree with organic bodies and disagree with all other things, entirely subordinate the minor distinctions: such distinctions being scarcely greater than those which separate one half of the organic kingdom from the other. The *principles* of organization are the same, and the differences are simply differences of application.

Here ending this general survey of the facts which justify the comparison of a society with a living body, let us look at them in detail. We shall find that the parallelism becomes the more marked the more closely it is examined.

The lowest animal and vegetal forms—*Protozoa* and *Protophyta*—are chiefly inhabitants of the water. They are minute bodies, most of which are made individually visible only by the microscope. All of them are extremely simple in structure, and some of them, as the *Rhizopods*, almost structureless. Multiplying, as they ordinarily do, by the spontaneous division of their bodies, they produce halves which may either become quite separate and move away in different directions, or may continue attached. By the repetition of this process of fission, aggregations of various sizes and kinds are formed. Among the *Protophyta* we have some classes, as the *Diatomaceæ* and the Yeast-plant, in which the individuals may be either separate or attached in groups of two, three, four, or more; other classes in which a considerable number of cells are united into a thread (*Conferva, Monilia*); others in which they form a network (*Hydrodictyon*); others in which they form plates (*Ulva*); and others in which they form masses (*Laminaria, Agaricus*): all which vegetal forms, having no distinction of root, stem, or leaf, are called *Thallogens*. Among the *Protozoa* we find parallel facts. Immense numbers of *Amœba*-like creatures, massed together in a framework of horny fibres, constitute Sponge. In the *Foraminifera* we see smaller groups of such creatures arranged into more definite shapes. Not only do these almost structureless *Protozoa* unite into regular or irregular aggregations of various sizes, but among some of the more organized ones, as the *Vorticellæ*, there are also produced clusters of individuals united to a common stem. But these little societies of monads, or cells, or whatever else we may call them, are societies only in the lowest sense: there is no subordination of parts among them—no organization. Each of the component units lives by and for itself; neither giving nor receiving aid. The only mutual dependence is that consequent on mechanical union.

Do we not here discern analogies to the first stages of human societies? Among the lowest races, as the Bushmen, we find but incipient aggregation: sometimes single families, sometimes two or three families wandering about together. The number of associated units is small and variable, and their union inconstant. No division of labour exists except between the sexes, and the only kind of mutual aid is that of joint attack or defence. We see an undifferentiated group of individuals, forming the germ of a society; just as in the homogeneous groups of cells above described, we see the initial stage of animal and vegetal organization.

The comparison may now be carried a step higher. In the vegetal kingdom we pass from the *Thallogens*, consisting of mere masses of similar cells, to the *Acrogens*, in which the cells are not similar throughout the whole mass; but are here aggregated into a structure serving as leaf and there into a structure serving as root; thus forming a whole in which there is a certain subdivision of functions among the units, and therefore a certain mutual dependence. In the animal kingdom we find analogous progress. From mere unorganized groups of cells, or cell-like bodies, we ascend to groups of such cells arranged into parts that have different duties. The common Polype, from the substance of which may be separated cells that exhibit, when detached, appearances and movements like those of a solitary *Amœba*, illustrates this stage. The component units, though still showing great community of character, assume somewhat diverse functions in the skin, in the internal surface, and in the tentacles. There is a certain amount of "physiological division of labour."

Turning to societies, we find these stages paralleled in most aboriginal tribes. When, instead of such small variable groups as are formed by Bushmen, we come to the larger and more permanent groups formed by savages not quite so low, we find traces of social structure. Though industrial organization scarcely shows itself, except in the different occupations of the sexes; yet there is more or less of governmental organization. While all the men are warriors and hunters, only a part of them are included in the council of chiefs; and in this council of chiefs some one has commonly supreme authority. There is thus a certain distinction of classes and powers; and through this slight specialization of functions is effected a rude co-operation among the increasing mass of individuals, whenever the society has to act in its corporate capacity. Beyond this analogy in the slight extent to which organization is carried, there is analogy in the indefiniteness of the organization. In the *Hydra,* the respective parts of the creature's substance have many functions in common. They are all contractile; omitting the tentacles, the whole of the external surface can give origin to young *hydræ;* and, when turned inside out, stomach performs the duties of skin and skin the duties of stomach. In aboriginal societies such differentiations as exist are similarly imperfect. Notwithstanding distinctions of rank, all persons maintain themselves by their own exertions. Not only do the head men of the tribe, in common with the rest, build their own huts, make their own weapons, kill their own food; but the chief does the like. Moreover, such governmental organization as exists is inconstant. It is frequently changed by violence or treachery, and the function of ruling assumed by some other warrior. Thus between the rudest societies and some of the lowest forms of animal life, there is analogy alike in the slight extent to which organization is carried, in the indefiniteness of this organization, and in its want of fixity.

A further complication of the analogy is at hand. From the aggregation of units into organized groups, we pass to the multiplication of such groups, and their coalescence into compound groups. The *Hydra,* when it has reached a certain bulk, puts forth from its surface a bud which, growing and gradually assuming the form of the parent, finally becomes detached; and by this process of gemmation the creature peoples the adjacent water with others like itself. A parallel process is seen in the multiplication of those lowly-organized tribes above described. When one of them has increased to a size that is either too great for co-ordination under so rude a structure, or else that is greater than the surrounding country can supply with game and other wild food, there arises a tendency to divide; and as in such communities there often occur quarrels, jealousies, and other causes of division, there soon comes an occasion on which a part of the tribe separates under the leadership of some subordinate chief and migrates. This process being from time to time repeated, an extensive region is at length occupied by numerous tribes descended from a common ancestry. The analogy by no means ends here. Though in the common *Hydra* the young ones that bud out from the parent soon become detached and independent; yet throughout the rest of the class *Hydrozoa*, to which this creature belongs, the like does not generally happen. The successive individuals thus developed continue attached; give origin to other such individuals which also continue attached; and so there results a compound animal. As in the *Hydra* itself we find an aggregation of units which, considered separately, are akin to the lowest *Protozoa;* so here, in a *Zoophyte*, we find an aggregation of such aggregations. The like is also seen throughout the extensive family of *Polyzoa* or *Molluscoida*. The Ascidian Mollusks, too, in their many forms, show us the same thing: exhibiting, at the same time, various degrees of union among the component individuals. For while in the *Salpæ* the component individuals adhere so slightly that a blow on the vessel of water in which they are floating will separate them; in the *Botryllidæ* there exist vascular connexions among them, and a common circulation. Now in these different stages of aggregation, may we not see paralleled the union of groups of connate tribes into nations? Though, in regions where circumstances permit, the tribes descended from some original tribe migrate in all directions, and become far removed and quite separate; yet, where the territory presents barriers to distant migration, this does not happen: the small kindred communities are held in closer contact, and eventually become more or less united into a nation. The contrast between the tribes of American Indians and the Scottish clans, illustrates this. And a glance at our own early history, or the early histories of continental nations, shows this fusion of small simple communities taking place in various ways and to various extents. As says M. Guizot, in his *History of the Origin of Representative Government,*—

> "By degrees, in the midst of the chaos of the rising society, small aggregations are formed which feel the want of alliance and union with each other. . . . Soon inequality of strength is displayed among neighbouring aggregations. The strong tend to subjugate the weak, and usurp at first the rights of taxation and military service. Thus political authority leaves the aggregations which first instituted it, to take a wider range."

That is to say, the small tribes, clans, or feudal groups, sprung mostly from a common stock, and long held in contact as occupants of adjacent lands, gradually get united in other ways than by kinship and proximity.

A further series of changes begins now to take place, to which, as before, we find analogies in individual organisms. Returning to the *Hydrozoa,* we observe that in the simplest of the compound forms the connected individuals are alike in structure, and perform like functions; with the exception that here and there a bud, instead of developing into a stomach, mouth, and tentacles, becomes an egg-sac. But with the oceanic *Hydrozoa* this is by no means the case. In the *Calycophoridæ* some of the polypes growing from the common germ, become developed and modified into large, long, sack-like bodies, which, by their rhythmical contractions, move through the water, dragging the community of polypes after them. In the *Physophoridæ* a variety of organs similarly arise by transformation of the budding polypes; so that in creatures like the *Physalia,* commonly known as the "Portuguese Man-of-war," instead of that tree-like group of similar individuals forming the original type, we have a complex mass of unlike parts fulfilling unlike duties. As an individual *Hydra* may be regarded as a group of *Protozoa* which have become partially metamorphosed into different organs; so a *Physalia* is, morphologically considered, a group of *Hydræ* of which the individuals have been variously transformed to fit them for various functions.

This differentiation upon differentiation is just what takes place during the evolution of a civilized society. We observed how, in the small communities first formed, there arises a simple political organization: there is a partial separation of classes having different duties. And now we have to observe how, in a nation formed by the fusion of such small communities, the several sections, at first alike in structures and modes of activity, grow unlike in both—gradually become mutually-dependent parts, diverse in their natures and functions.

The doctrine of the progressive division of labour, to which we are here introduced, is familiar to all readers. And further, the analogy between the economical division of labour and the "physiological division of labour," is so striking as long since to have drawn the attention of scientific naturalists: so striking, indeed, that the expression "physiological division of labour," has been suggested by it. It is not needful, therefore, to treat this part of the subject in great detail. We shall content ourselves with noting a few general and significant facts, not manifest on a first inspection.

Throughout the whole animal kingdom, from the *Cœlenterata* upwards, the first stage of evolution is the same. Equally in the germ of a polype and in the human ovum, the aggregated mass of cells out of which the creature is to arise, gives origin to a peripheral layer of cells, slightly differing from the rest which they include; and this layer subsequently divides into two—the inner, lying in contact with the included yelk, being called the mucous layer, and the outer, exposed to surrounding agencies, being called the serous layer: or, in the terms used by Prof. Huxley, in describing the development of the *Hydrozoa*—the endoderm and ectoderm. This primary division marks out a fundamental contrast of parts in the future organism. From the mucous layer, or endoderm, is developed the apparatus of nutrition; while from the serous layer, or ectoderm, is developed the apparatus of external action. Out of the one arise the organs by which food is prepared and absorbed, oxygen imbibed, and blood purified; while out of the other arise the nervous, muscular, and osseous systems, by the combined actions of which the movements of the body as a whole are effected. Though this is not a rigorously-correct distinction, seeing that some organs involve both of these primitive membranes, yet high authorities agree in stating it as a broad general distinction. Well, in the evolution of a society, we see a primary differentiation of analogous kind, which similarly underlies the whole future structure. As already pointed out, the only manifest contrast of parts in primitive societies, is that between the governing and the governed. In the least organized tribes, the council of chiefs may be a body of men distinguished simply by greater courage or experience. In more organized tribes, the chief-class is definitely separated from the lower class, and often regarded as different in nature—sometimes as god-descended. And later, we find these two becoming respectively freemen and slaves, or nobles and serfs. A glance at their respective functions, makes it obvious that the great divisions thus early formed, stand to each other in a relation similar to that in which the primary divisions of the embryo stand to each other. For, from its first appearance, the warrior-class, headed by chiefs, is that by which the external acts of the society are carried on: alike in war, in negotiation, and in migration. Afterwards, while this upper class grows distinct from the lower, and at the same time becomes more and more exclusively regulative and defensive in its functions, alike in the persons of kings and subordinate rulers, priests, and soldiers; the inferior class becomes more and more exclusively occupied in providing the necessaries of life for the community at large. From the soil, with which it comes in most direct contact, the mass of the people takes up, and prepares for use, the food and such rude articles of manufacture as are known; while the overlying mass of superior men, maintained by the working population, deals with circumstances external to the community—circumstances with which, by position, it is more immediately concerned. Ceasing by-and-by to have any knowledge of, or power over, the concerns of the society as a whole, the serf-class becomes devoted to the processes of alimentation; while the noble class, ceasing to take any part in the processes of alimentation, becomes devoted to the co-ordinated movements of the entire body-politic.

Equally remarkable is a further analogy of like kind. After the mucous and serous layers of the embryo have separated, there presently arises between the two a third, known to physiologists as the vascular layer—a layer out of which are developed the chief blood-vessels. The mucous layer absorbs nutriment from the mass of yelk it encloses; this nutriment has to be transferred to the overlying serous layer, out of which the nervo-muscular system is being developed; and between the two arises a vascular system by which the transfer is effected—a system of vessels which continues ever after to be the transferrer of nutriment from the places where it is absorbed and prepared, to the places where it is needed for growth and repair. Well, may we not trace a parallel step in social progress? Between the governing and the governed, there at first exists no intermediate class; and even in some societies that have reached considerable sizes, there are scarcely any but the nobles and their kindred on the one hand, and the serfs on the other: the social structure being such that transfer of commodities takes place directly from slaves to their masters. But in societies of a higher type, there grows up, between these two primitive classes, another—the trading or middle class. Equally at first as now, we may see that, speaking generally, this middle class is the analogue of the middle layer in the embryo. For all traders are essentially distributors. Whether they be wholesale dealers, who collect into large masses the commodities of various producers; or whether they be retailers, who divide out to those who want them, the masses of commodities thus collected together; all mercantile men are agents of transfer from the places where things are produced to the places where they are consumed. Thus the distributing apparatus in a society, answers to the distributing apparatus in a living body; not only in its functions, but in its intermediate origin and subsequent position, and in the time of its appearance.

Without enumerating the minor differentiations which these three great classes afterwards undergo, we will merely note that throughout, they follow the same general law with the differentiations of an individual organism. In a society, as in a rudimentary animal, we have seen that the most general and broadly contrasted divisions are the first to make their appearance; and of the subdivisions it continues true in both cases, that they arise in the order of decreasing generality.

Let us observe, next, that in the one case as in the other, the specializations are at first very incomplete, and approach completeness as organization progresses. We saw that in primitive tribes, as in the simplest animals, there remains much community of function between the parts which are nominally different—that, for instance, the class of chiefs long remains industrially the same as the inferior class; just as in a *Hydra*, the property of contractility is possessed by the units of the endoderm as well as by those of the ectoderm. We noted also how, as the society advanced, the two great primitive classes partook less and less of each other's functions. And we have here to remark that all subsequent specializations are at first vague and gradually become distinct. "In the infancy of society," says M. Guizot, "everything is confused and uncertain; there is as yet no fixed and precise line of demarcation between the different powers in a state." "Originally kings lived like other landowners, on the incomes derived from their own private estates." Nobles were petty kings; and kings only the most powerful nobles. Bishops were feudal lords and military leaders. The right of coining money was possessed by powerful subjects, and by the Church, as well as by the king. Every leading man exercised alike the functions of landowner, farmer, soldier, statesman, judge. Retainers were now soldiers, and now labourers, as the day required. But by degrees the Church has lost all civil jurisdiction; the State has exercised less and less control over religious teaching; the military class has grown a distinct one; handicrafts have concentrated in towns; and the spinning-wheels of scattered farmhouses, have disappeared before the machinery of manufacturing districts. Not only is all progress from the homogeneous to the heterogeneous, but, at the same time, it is from the indefinite to the definite.

Another fact which should not be passed over, is that in the evolution of a large society out of a cluster of small ones, there is a gradual obliteration of the original lines of separation—a change to which, also, we may see analogies in living bodies. The sub-kingdom *Annulosa*, furnishes good illustrations. Among the lower types the body consists of numerous segments that are alike in nearly every particular. Each has its external ring; its pair of legs, if the creature has legs; its equal portion of intestine, or else its separate stomach; its equal portion of the great blood-vessel, or, in some cases, its separate heart; its equal portion of the nervous cord; and, perhaps, its separate pair of ganglia. But in the highest types, as in the large *Crustacea,* many of the segments are completely fused together; and the internal organs are no longer uniformly repeated in all the segments. Now the segments of which nations at first consist, lose their separate external and internal structures in a similar manner. In feudal times the minor communities, governed by feudal lords, were severally organized in the same rude way, and were held together only by the fealty of their respective rulers to a suzerain. But along with the growth of a central power, the demarcations of these local communities become relatively unimportant, and their separate organizations merge into the general organization. The like is seen on a larger scale in the fusion of England, Wales, Scotland, and Ireland; and, on the Continent, in the coalescence of provinces into kingdoms. Even in the disappearance of law-made divisions, the process is analogous. Among the Anglo-Saxons, England was divided into tithings, hundreds, and counties: there were county-courts, courts of hundred, and courts of tithing. The courts of tithing disappeared first; then the courts of hundred, which have, however, left traces; while the county-jurisdiction still exists. Chiefly, however, it is to be noted, that there eventually grows up an organization which has no reference to these original divisions, but traverses them in various directions, as is the case in creatures belonging to the sub-kingdom just named; and, further, that in both cases it is the sustaining organization which thus traverses old boundaries, while, in both cases, it is the governmental, or co-ordinating organization in which the original boundaries continue traceable. Thus, in the highest *Annulosa* the exo-skeleton and the muscular system never lose all traces of their primitive segmentation; but throughout a great part of the body, the contained viscera do not in the least conform to the external divisions. Similarly with a nation we see that while, for governmental purposes, such divisions as counties and parishes still exist, the structure developed for carrying on the nutrition of society wholly ignores these boundaries: our great cotton-manufacture spreads out of Lancashire into North Derbyshire; Leicestershire and Nottinghamshire have long divided the stocking-trade between them; one great centre for the production of iron and iron-goods, includes parts of Warwickshire, Staffordshire, and Worcestershire; and those various specializations of agriculture which have made different parts of England noted for different products, show no more respect to county-boundaries than do our growing towns to the boundaries of parishes.

If, after contemplating these analogies of structure, we inquire whether there are any such analogies between the processes of organic change, the answer is—yes. The causes which lead to increase of bulk in any part of the body-politic, are of like nature with those which lead to increase of bulk in any part of an individual body. In both cases the antecedent is greater functional activity consequent on greater demand. Each limb, viscus, gland, or other member of an animal, is developed by exercise—by actively discharging the duties which the body at large requires of it; and similarly, any class of labourers or artisans, any manufacturing centre, or any official agency, begins to enlarge when the community devolves on it more work. In each case, too, growth has its conditions and its limits. That any organ in a living being may grow by exercise, there needs a due supply of blood. All action implies waste; blood brings the materials for repair; and before there can be growth, the quantity of blood supplied must be more than is requisite for repair. In a society it is the same. If to some district which elaborates for the community particular commodities—say the woollens of Yorkshire—there comes an augmented demand; and if, in fulfilment of this demand, a certain expenditure and wear of the manufacturing organization are incurred; and if, in payment for the extra quantity of woollens sent away, there comes back only such quantity of commodities as replaces the expenditure, and makes good the waste of life and machinery; there can clearly be no growth. That there may be growth, the commodities obtained in return must be more than sufficient for these ends; and just in proportion as the surplus is great will the growth be rapid. Whence it is manifest that what in commercial affairs we call *profit*, answers to the excess of nutrition over waste in a living body. Moreover, in both cases when the functional activity is high and the nutrition defective, there results not growth but decay. If in an animal, any organ is worked so hard that the channels which bring blood cannot furnish enough for repair, the organ dwindles: atrophy is set up. And if in the body-politic, some part has been stimulated into great productivity, and cannot afterwards get paid for all its produce, certain of its members become bankrupt, and it decreases in size.

One more parallelism to be here noted, is that the different parts of a social organism, like the different parts of an individual organism, compete for nutriment; and severally obtain more or less of it according as they are discharging more or less duty. If a man's brain be over-excited it abstracts blood from his viscera and stops digestion; or digestion, actively going on, so affects the circulation through the brain as to cause drowsiness; or great muscular exertion determines such a quantity of blood to the limbs as to arrest digestion or cerebral action, as the case may be. So, likewise, in a society, great activity in some one direction causes partial arrests of activity elsewhere by abstracting capital, that is commodities: as instance the way in which the sudden development of our railway-system hampered commercial operations; or the way in which the raising of a large military force temporarily stops the growth of leading industries.

The last few paragraphs introduce the next division of our subject. Almost unawares we have come upon the analogy which exists between the blood of a living body and the circulating mass of commodities in the body-politic. We have now to trace out this analogy from its simplest to its most complex manifestations.

In the lowest animals there exists no blood properly so called. Through the small assemblage of cells which make up a *Hydra*, permeate the juices absorbed from the food. There is no apparatus for elaborating a concentrated and purified nutriment, and distributing it among the component units; but these component units directly inbibe the unprepared nutriment, either from the digestive cavity or from one another. May we not say that this is what takes place in an aboriginal tribe? All its members severally obtain for themselves the necessaries of life in their crude states; and severally prepare them for their own uses as well as they can. When there arises a decided differentiation between the governing and the governed, some amount of transfer begins between those inferior individuals who, as workers, come directly in contact with the products of the earth, and those superior ones who exercise the higher functions—a transfer parallel to that which accompanies the differentiation of the ectoderm from the endoderm. In the one case, as in the other, however, it is a transfer of products that are little if at all prepared; and takes place directly from the unit which obtains to the unit which consumes, without entering into any general current.

Passing to larger organisms—individual and social—we meet the first advance on this arrangement. Where, as among the compound *Hydrozoa*, there is a union of many such primitive groups as form *Hydræ;* or where, as in a *Medusa*, one of these groups has become of great size; there exist rude channels running throughout the substance of the body: not, however, channels for the conveyance of prepared nutriment, but mere prolongations of the digestive cavity, through which the crude chyle-aqueous fluid reaches the remoter parts, and is moved backwards and forwards by the creature's contractions. Do we not find in some of the more advanced primitive communities an analogous condition? When the men, partially or fully united into one society, become numerous—when, as usually happens, they cover a surface of country not everywhere alike in its products—when, more especially, there arise considerable classes which are not industrial; some process of exchange and distribution inevitably arises. Traversing here and there the earth's surface, covered by that vegetation on which human life depends, and in which, as we say, the units of a society are imbedded, there are formed indefinite paths, along which some of the necessaries of life occasionally pass, to be bartered for others which presently come back along the same channels. Note, however, that at first little else but crude commodities are thus transferred—fruits, fish, pigs or cattle, skins, etc.: there are few, if any, manufactured products or articles prepared for consumption. And note also, that such distribution of these unprepared necessaries of life as takes place, is but occasional—goes on with a certain slow, irregular rhythm.

Further progress in the elaboration and distribution of nutriment, or of commodities, is a necessary accompaniment of further differentiation of functions in the individual body or in the body-politic. As fast as each organ of a living animal becomes confined to a special action, it must become dependent on the rest for those materials which its position and duty do not permit it to obtain for itself; in the same way that, as fast as each particular class of a community becomes exclusively occupied in producing its own commodity, it must become dependent on the rest for the other commodities it needs. And, simultaneously, a more perfectly-elaborated blood will result from a highly specialized group of nutritive organs, severally adapted to prepare its different elements; in the same way that the stream of commodities circulating throughout a society, will be of superior quality in proportion to the greater division of labour among the workers. Observe, also, that in either case the circulating mass of nutritive materials, besides coming gradually to consist of better ingredients, also grows more complex. An increase in the number of the unlike organs which add to the blood their waste matters, and demand from it the different materials they severally need, implies a blood more heterogeneous in composition—an *a priori* conclusion which, according to Dr. Williams, is inductively confirmed by examination of the blood throughout the various grades of the animal kingdom. And similarly, it is manifest that as fast as the division of labour among the classes of a community becomes greater, there must be an increasing heterogeneity in the currents of merchandize flowing throughout that community.

The circulating mass of nutritive materials in individual organisms and in social organisms, becoming at once better in the quality of its ingredients and more heterogeneous in composition, as the type of structure becomes higher, eventually has added to it in both cases another element, which is not itself nutritive but facilitates the processes of nutrition. We refer, in the case of the individual organism, to the blood-discs; and in the case of the social organism, to money. This analogy has been observed by Liebig, who in his *Familiar Letters on Chemistry* says:—

> "Silver and gold have to perform in the organism of the state, the same function as the blood-corpuscles in the human organism. As these round discs, without themselves taking an immediate share in the nutritive process, are the medium, the essential condition of the change of matter, of the production of the heat and of the force by which the temperature of t

**body is kept up, and the motions of the blood and all the juices are determined, so has gold become the medium of all activity in the life of the state."**

And blood-corpuscles being like coin in their functions, and in the fact that they are not consumed in nutrition, he further points out that the number of them which in a considerable interval flows through the great centres, is enormous when compared with their absolute number; just as the quantity of money which annually passes through the great mercantile centres, is enormous when compared with the quantity of money in the kingdom. Nor is this all. Liebig has omitted the significant circumstance that only at a certain stage of organization does this element of the circulation make its appearance. Throughout extensive divisions of the lower animals, the blood contains no corpuscles; and in societies of low civilization, there is no money.

Thus far we have considered the analogy between the blood in a living body and the consumable and circulating commodities in the body-politic. Let us now compare the appliances by which they are respectively distributed. We shall find in the developments of these appliances parallelisms not less remarkable than those above set forth. Already we have shown that, as classes, wholesale and retail distributors discharge in a society the office which the vascular system discharges in an individual creature; that they come into existence later than the other two great classes, as the vascular layer appears later than the mucous and serous layers; and that they occupy a like intermediate position. Here, however, it remains to be pointed out that a complete conception of the circulating system in a society, includes not only the active human agents who propel the currents of commodities, and regulate their distribution, but includes, also, the channels of communication. It is the formation and arrangement of these to which we now direct attention.

Going back once more to those lower animals in which there is found nothing but a partial diffusion, not of blood, but only of crude nutritive fluids, it is to be remarked that the channels through which the diffusion takes place, are mere excavations through the half-organized substance of the body: they have no lining membranes, but are mere *lacunæ* traversing a rude tissue. Now countries in which civilization is but commencing, display a like condition: there are no roads properly so called; but the wilderness of vegetal life covering the earth's surface is pierced by tracks, through which the distribution of crude commodities takes place. And while, in both cases, the acts of distribution occur only at long intervals (the currents, after a pause, now setting towards a general centre and now away from it), the transfer is in both cases slow and difficult. But among other accompaniments of progress, common to animals and societies, comes the formation of more definite and complete channels of communication. Blood-vessels acquire distinct walls; roads are fenced and gravelled. This advance is first seen in those roads or vessels that are nearest to the chief centres of distribution; while the peripheral roads and peripheral vessels long continue in their primitive states. At a yet later stage of development, where comparative finish of structure is found throughout the system as well as near the chief centres, there remains in both cases the difference that the main channels are comparatively broad and straight, while the subordinate ones are narrow and tortuous in proportion to their remoteness. Lastly, it is to be remarked that there ultimately arise in the higher social organisms, as in the higher individual organisms, main channels of distribution still more distinguished by their perfect structures, their comparative straightness, and the absence of those small branches which the minor channels perpetually give off. And in railways we also see, for the first time in the social organism, a system of double channels conveying currents in opposite directions, as do the arteries and veins of a well-developed animal.

These parallelisms in the evolutions and structures of the circulating systems, introduce us to others in the kinds and rates of the movements going on through them. Through the lowest societies, as through the lowest creatures, the distribution of crude nutriment is by slow gurgitations and regurgitations. In creatures that have rude vascular systems, just as in societies that are beginning to have roads, there is no regular circulation along definite courses; but, instead, periodical changes of the currents—now towards this point and now towards that. Through each part of an inferior mollusk's body, the blood flows for a while in one direction, then stops and flows in the opposite direction; just as through a rudely-organized society, the distribution of merchandize is slowly carried on by great fairs, occurring in different localities, to and from which the currents periodically set. Only animals of tolerably complete organizations, like advanced communities, are permeated by constant currents that are definitely directed. In living bodies, the local and variable currents disappear when there grow up great centres of circulation, generating more powerful currents by a rhythm which ends in a quick, regular pulsation. And when in social bodies there arise great centres of commercial activity, producing and exchanging large quantities of commodities, the rapid and continuous streams drawn in and emitted by these centres subdue all minor and local circulations: the slow rhythm of fairs merges into the faster one of weekly markets, and in the chief centres of distribution, weekly markets merge into daily markets; while in place of the languid transfer from place to place, taking place at first weekly, then twice or thrice a week, we by-and-by get daily transfer, and finally transfer many times a day—the original sluggish, irregular rhythm, becomes a rapid, equable pulse. Mark, too, that in both cases the increased activity, like the greater perfection of structure, is much less conspicuous at the periphery of the vascular system. On main lines of railway, we have, perhaps, a score trains in each direction daily, going at from thirty to fifty miles an hour; as, through the great arteries, the blood moves rapidly in successive gushes. Along high roads, there go vehicles conveying men and commodities with much less, though still considerable, speed, and with a much less decided rhythm; as, in the smaller arteries, the speed of the blood is greatly diminished and the pulse less conspicuous. In parish-roads, narrower, less complete, and more tortuous, the rate of movement is further decreased and the rhythm scarcely traceable; as in the ultimate arteries. In those still more imperfect by-roads which lead from these parish-roads to scattered farmhouses and cottages, the motion is yet slower and very irregular; just as we find it in the capillaries. While along the field-roads, which, in their unformed, unfenced state, are typical of *lacunæ*, the movement is the slowest, the most irregular, and the most infrequent; as it is, not only in the primitive *lacunæ* of animals and societies, but as it is also in those *lacunæ* in which the vascular system ends among extensive families of inferior creatures.

Thus, then, we find between the distributing systems of living bodies and the distributing systems of bodies-politic, wonderfully close parallelisms. In the lowest forms of individual and social organisms, there exist neither prepared nutritive matters nor distributing appliances; and in both, these, arising as necessary accompaniments of the differentiation of parts, approach perfection as this differentiation approaches completeness. In animals, as in societies, the distributing agencies begin to show themselves at the same relative periods, and in the same relative positions. In the one, as in the other, the nutritive materials circulated are at first crude and simple, gradually become better elaborated and more heterogeneous, and have eventually added to them a new element facilitating the nutritive processes. The channels of communication pass through similar phases of development, which bring them to analogous forms. And the directions, rhythms, and rates of circulation, progress by like steps to like final conditions.

We come at length to the nervous system. Having noticed the primary differentiation of societies into the governing and governed classes, and observed its analogy to the differentiation of the two primary tissues which respectively develop into organs of external action and organs of alimentation; having noticed some of the leading analogies between the development of industrial arrangements and that of the alimentary apparatus; and having, above, more fully traced the analogies between the distributing systems, social and individual; we have now to compare the appliances by which a society, as a whole, is regulated, with those by which the movements of an individual creature are regulated. We shall find here parallelisms equally striking with those already detailed.

The class out of which governmental organization originates, is, as we have said, analogous in its relations to the ectoderm of the lowest animals and of embryonic forms. And as this primitive membrane, out of which the nervo-muscular system is evolved, must, even in the first stage of its differentiation, be slightly distinguished from the rest by that greater impressibility and contractility characterizing the organs to which it gives rise; so, in that superior class which is eventually transformed into the directo-executive system of a society (its legislative and defensive appliances), does there exist in the beginning, a larger endowment of the capacities required for these higher social functions. Always, in rude assemblages of men, the strongest, most courageous, and most sagacious, become rulers and leaders; and, in a tribe of some standing, this results in the establishment of a dominant class, characterized on the average by those mental and bodily qualities which fit them for deliberation and vigorous combined action. Thus that greater impressibility and contractility, which in the rudest animal types characterize the units of the ectoderm, characterize also the units of the primitive social stratum which controls and fights; since impressibility and contractility are the respective roots of intelligence and strength.

Again, in the unmodified ectoderm, as we see it in the *Hydra,* the units are all endowed both with impressibility and contractility; but as we ascend to higher types of organization, the ectoderm differentiates into classes of units which divide those two functions between them: some, becoming exclusively impressible, cease to be contractile; while some, becoming exclusively contractile, cease to be impressible. Similarly with societies. In an aboriginal tribe, the directive and executive functions are diffused in a mingled form throughout the whole governing class. Each minor chief commands those under him, and, if need be, himself coerces them into obedience. The council of chiefs itself carries out on the battle-field its own decisions. The head chief not only makes laws, but administers justice with his own hands. In larger and more settled communities, however, the directive and executive agencies begin to grow distinct from each other. As fast as his duties accumulate, the head chief or king confines himself more and more to directing public affairs, and leaves the execution of his will to others: he deputes others to enforce submission, to inflict punishments, or to carry out minor acts of offence and defence; and only on occasions when, perhaps, the safety of the society and his own supremacy are at stake, does he begin to act as well as direct. As this differentiation establishes itself, the characteristics of the ruler begin to change. No longer, as in an aboriginal tribe, the strongest and most daring man, the tendency is for him to become the man of greatest cunning, foresight, and skill in the management of others; for in societies that have advanced beyond the first stage, it is chiefly such qualiti that insure success in gaining supreme power, and holding it against internal and external enemies. Thus that member of the governing class who comes to be the chief directing agent, and so plays the same part that a rudimentary nervous centre does in an unfolding organism, is usually one endowed with some superiorities of nervous organization.

In those larger and more complex communities possessing, perhaps, a separate military class, a priesthood, and dispersed masses of population requiring local control, there grow up subordinate governing agents; who, as their duties accumulate, severally become more directive and less executive in their characters. And when, as commonly happens, the king begins to collect round himself advisers who aid him by communicating information, preparing subjects for his judgment, and issuing his orders; we may say that the form of organization is comparable to one very general among inferior types of animals, in which there exists a chief ganglion with a few dispersed minor ganglia under its control.

The analogies between the evolution of governmental structures in societies, and the evolution of governmental structures in living bodies, are, however, more strikingly displayed during the formation of nations by coalescence of tribes—a process already shown to be, in several respects, parallel to the development of creatures that primarily consist of many like segments. Among other points of community between the successive rings which make up the body in the lower *Annulosa,* is the possession of similar pairs of ganglia. These pairs of ganglia, though connected by nerves, are very incompletely dependent on any general controlling power. Hence it results that when the body is cut in two, the hinder part continues to move forward under the propulsion of its numerous legs; and that when the chain of ganglia has been divided without severing the body, the hind limbs may be seen trying to propel the body in one direction while the fore limbs are trying to propel it in another. But in the higher *Annulosa,* called *Articulata,* sundry of the anterior pairs of ganglia, besides growing larger, unite in one mass; and this great cephalic ganglion having become the co-ordinator of all the creature's movements, there no longer exists much local independence. Now may we not in the growth of a consolidated kingdom out of petty sovereignties or baronies, observe analogous changes? Like the chiefs and primitive rulers above described, feudal lords, exercising supreme power over their respective groups of retainers, discharge functions analogous to those of rudimentary nervous centres. Among these local governing centres there is, in early feudal times, very little subordination. They are in frequent antagonism; they are individually restrained chiefly by the influence of parties in their own class; and they are but irregularly subject to that most powerful member of their order who has gained the position of head-suzerain or king. As the growth and organization of the society progresses, these local directive centres fall more and more under the control of a chief directive centre. Closer commercial union between the several segments is accompanied by closer governmental union; and these minor rulers end in being little more than agents who administer, in their several localities, the laws made by the supreme ruler: just as the local ganglia above described, eventually become agents which enforce, in their respective segments, the orders of the cephalic ganglion. The parallelism holds still further. We remarked above, when speaking of the rise of aboriginal kings, that in proportion as their territories increase, they are obliged not only to perform their executive functions by deputy, but also to gather round themselves advisers to aid in their directive functions; and that thus, in place of a solitary governing unit, there grows up a group of governing units, comparable to a ganglion consisting of many cells. Let us here add that the advisers and chief officers who thus form the rudiment of a ministry, tend from the beginning to exercise some control over the ruler. By the information they give and the opinions they express, they sway his judgment and affect his commands. To this extent he is made a channel through which are communicated the directions originating with them; and in course of time, when the advice of ministers becomes the acknowledged source of his actions, the king assumes the character of an automatic centre, reflecting the impressions made on him from without.

Beyond this complication of governmental structure many societies do not progress; but in some, a further development takes place. Our own case best illustrates this further development and its further analogies. To kings and their ministries have been added, in England, other great directive centres, exercising a control which, at first small, has been gradually becoming predominant: as with the great governing ganglia which especially distinguish the highest classes of living beings. Strange as the assertion will be thought, our Houses of Parliament discharge, in the social economy, functions which are in sundry respects comparable to those discharged by the cerebral masses in a vertebrate animal. As it is in the nature of a single ganglion to be affected only by special stimuli from particular parts of the body; so it is in the nature of a single ruler to be swayed in his acts by exclusive personal or class interests. As it is in the nature of a cluster of ganglia, connected with the primary one, to convey to it a greater variety of influences from more numerous organs, and thus to make its acts conform to more numerous requirements; so it is in the nature of the subsidiary controlling powers surrounding a king to adapt his rule to a greater number of public exigencies. And as it is in the nature of those great and latest-developed ganglia which distinguish the higher animals, to interpret and combine the multiplied and varied impressions conveyed to them from all parts of the system, and to regulate the actions in such way as duly to regard them all; so it is in the nature of those great and latest-developed legislative bodies which distinguish the most advanced societies, to interpret and combine the wishes of all classes and localities, and to make laws in harmony with the general wants. We may describe the office of the brain as that of *averaging* the interests of life, physical, intellectual, moral; and a good brain is one in which the desires answering to these respective interests are so balanced, that the conduct they jointly dictate, sacrifices none of them. Similarly, we may describe the office of a Parliament as that of *averaging* the interests of the various classes in a community; and a good Parliament is one in which the parties answering to these respective interests are so balanced, that their united legislation allows to each class as much as consists with the claims of the rest. Besides being comparable in their duties, these great directive centres, social and individual, are comparable in the processes by which their duties are discharged. The cerebrum is not occupied with direct impressions from without but with the ideas of such impressions. Instead of the actual sensations produced in the body, and directly appreciated by the sensory ganglia, or primitive nervous centres, the cerebrum receives only the representations of these sensations; and its consciousness is called *representative* consciousness, to distinguish it from the original or *presentative* consciousness. Is it not significant that we have hit on the same word to distinguish the function of our House of Commons? We call it a *representative* body, because the interests with which it deals are not directly presented to it, but represented to it by its various members; and a debate is a conflict of representations of the results likely to follow from a proposed course—a description which applies with equal truth to a debate in the individual consciousness. In both cases, too, these great governing masses take no part in the executive functions. As, after a conflict in the cerebrum, those desires which finally predominate act on the subjacent ganglia, and through their instrumentality determine the bodily actions; so the parties which, after a parliamentary struggle, gain the victory, do not themselves carry out their wishes, but get them carried out by the executive divisions of the Government. The fulfilment of all legislative decisions still devolves on the original directive centres: the impulse passing from the Parliament to the Ministers and from the Ministers to the King, in whose name everything is done; just as those smaller, first-developed ganglia, which in the lowest vertebrata are the chief controlling agents, are still, in the brains of the higher vertebrata, the agents through which the dictates of the cerebrum are worked out. Moreover, in both cases these original centres become increasingly automatic. In the developed vertebrate animal, they have little function beyond that of conveying impressions to, and executing the determinations of, the larger centres. In our highly organized government, the monarch has long been lapsing into a passive agent of Parliament; and now, ministries are rapidly falling into the same position. Nay, between the two cases there is a parallelism even in respect of the exceptions to this automatic action. For in the individual creature it happens that under circumstances of sudden alarm, as from a loud sound close at hand, an unexpected object starting up in front, or a slip from insecure footing, the danger is guarded against by some quick involuntary jump, or adjustment of the limbs, which occurs before there is time to consider the impending evil and take deliberate measures to avoid it: the rationale of which is that these violent impressions produced on the senses, are reflected from the sensory ganglia to the spinal cord and muscles, without, as in ordinary cases, first passing through the cerebrum. In like manner on national emergencies calling for prompt action, the King and Ministry, not having time to lay the matter before the great deliberative bodies, themselves issue commands for the requisite movements or precautions: the primitive, and now almost automatic, directive centres, resume for a moment their original uncontrolled power. And then, strangest of all, observe that in either case there is an after-process of approval or disapproval. The individual on recovering from his automatic start, at once contemplates the cause of his fright; and, according to the case, concludes that it was well he moved as he did, or condemns himself for his groundless alarm. In like manner, the deliberative powers of the State discuss, as soon as may be, the unauthorized acts of the executive powers; and, deciding that the reasons were or were not sufficient, grant or withhold a bill of indemnity.*

Thus far in comparing the governmental organization of the body-politic with that of an individual body, we have considered only the respective co-ordinating centres. We have yet to consider the channels through which these co-ordinating centres receive information and convey commands. In the simplest societies, as in the simplest organisms, there is no "internuncial apparatus," as Hunter styled the nervous system. Consequently, impressions can be but slowly propagated from unit to unit throughout the whole mass. The same progress, however, which, in animal-organization, shows itself in the establishment of ganglia or directive centres, shows itself also in the establishment of nerve-threads, through which the ganglia receive and convey impressions and so control remote organs. And in societies the like eventually takes place. After a long period during which the directive centres communicate with various parts of the society through other means, there at last comes into existence an "internuncial apparatus," analogous to that found in individual bodies. The comparison of telegraph-wires to nerves is familiar to all. It applies, however, to an extent not commonly supposed. Thus, throughout the vertebrate sub-kingdom, the great nerve-bundles diverge from the vertebrate axis side by side with the great arteries; and similarly, our groups of telegraph-wires are carried along the sides of our railways. The most striking parallelism, however, remains. Into each great bundle of nerves, as it leaves the axis of the body along with an artery, there enters a branch of the sympathetic nerve; which branch, accompanying the artery throughout its ramifications, has the function of regulating its diameter and otherwise controlling the flow of blood through it according to local requirements. Analogously, in the group of telegraph-wires running alongside each railway, there is a wire for the purpose of regulating the traffic—for retarding or expediting the flow of passengers and commodities, as the local conditions demand. Probably, when our now rudimentary telegraph-system is fully developed, other analogies will be traceable.

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* It may be well to warn the reader against an error fallen into by one who criticised this essay on its first publication—the error of supposing that the analogy here intended to be drawn, is a specific analogy between the organization of society in England, and the human organization. As said at the outset, no such specific analogy exists. The above parallel is one between the most-developed systems of governmental organization, individual and social; and the vertebrate type is instanced merely as exhibiting this most-developed system. If any specific comparison were made, which it cannot rationally be, it would be made with some much lower vertebrate form than the human.

Such, then, is a general outline of the evidence which justifies the comparison of societies to living organisms. That they gradually increase in mass; that they become little by little more complex; that at the same time their parts grow more mutually dependent; and that they continue to live and grow as wholes, while successive generations of their units appear and disappear; are broad peculiarities which bodies-politic display in common with all living bodies; and in whic hthey and living bodies differ from everything else. And on carrying out the comparison in detail, we find that these major analogies involve many minor analogies, far closer than might have been expected.

Others might be added. We had hoped to say something respecting the different types of social organization, and something also on social metamorphoses; but we have reached our assigned limits.

# THE ORIGIN OF ANIMAL WORSHIP.

[*First published in* The Fortnightly Review *for May*, 1870.]

MR. MCLENNAN'S recent essays on the Worship of Animals and Plants have done much to elucidate a very obscure subject. By pursuing in this case, as before in another case, the truly scientific method of comparing the phenomena presented by existing uncivilized races with those which the traditions of civilized races present, he has rendered both of them more comprehensible than they were before.

It seems to me, however, that Mr. McLennan gives but an indefinite answer to the essential question—How did the worship of animals and plants arise? Indeed, in his concluding paper, he expressly leaves this problem unsolved; saying that his "is not an hypothesis explanatory of the origin of *Totemism*, be it remembered, but an hypothesis explanatory of the animal and plant worship of the ancient nations." So that we have still to ask—Why have savage tribes so generally taken animals and plants and other things as totems? What can have induced this tribe to ascribe special sacredness to one creature, and that tribe to another? And if to these questions the reply is, that each tribe considers itself to be descended from the object of its reverence, then there presses for answer the further question —How came so strange a notion into existence? If this notion occurred in one case only, we might set it down to some whim of thought or some illusive occurrence. But appearing, as it does, with multitudinous variations among so many uncivilized races in different parts of the world, and having left numerous marks in the superstitions of extinct civilized races, we cannot assume any special or exceptional cause. Moreover, the general cause, whatever it may be, must be such as does not negative an aboriginal intelligence like in nature to our own. After studying the grotesque beliefs of savages, we are apt to suppose that their reason is not as our reason. But this supposition is inadmissible. Given the amount of knowledge which primitive men possess, and given the imperfect verbal symbols used by them in speech and thought, and the conclusions they habitually reach will be those that are *relatively* the most rational. This must be our postulate; and, setting out with this postulate, we have to ask how primitive men came so generally, if not universally, to believe themselves the progeny of animals or plants or inanimate bodies. There is, I believe, a satisfactory answer.

The proposition with which Mr. McLennan sets out, that totem-worship preceded the worship of anthropomorphic gods, is one to which I can yield but a qualified assent. It is true in a sense, but not wholly true. If the words "gods" and "worship" carry with them their ordinary definite meanings, the statement is true; but if their meanings are widened so as to comprehend those earliest vague notions out of which the definite ideas of gods and worship are evolved, I think it is not true. The rudimentary form of all religion is the propitiation of dead ancestors, who are supposed to be still existing, and to be capable of working good or evil to their descendants. As a preparation for dealing hereafter with the principles of sociology, I have, for some years past, directed much attention to the modes of thought current in the simpler human societies; and evidence of many kinds, furnished by all varieties of uncivilized men, has forced on me a conclusion harmonizing with that lately expressed in this Review by Prof. Huxley—namely, that the savage, conceiving a corpse to be deserted by the active personality who dwelt in it, conceives this active personality to be still existing, and that his feelings and ideas concerning it form the basis of his superstitions. Everywhere we find expressed or implied the belief that each person is double; and that when he dies, his other self, whether remaining near at hand or gone far away, may return, and continues capable of injuring his enemies and aiding his friends.*

But how out of the desire to propitiate this second personality of a deceased man (the words "ghost" and "spirit" are somewhat misleading, since the savage believes that the second personality reappears in a form equally tangible with the first), does there grow up the worship of animals, plants, and inanimate objects? Very simply. Savages habitually distinguish individuals by names that are either directly suggestive of some personal trait or fact of personal

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* A critical reader may raise an objection. If animal-worship is to be rationally interpreted, how can the interpretation set out by assuming a belief in the spirits of dead ancestors—a belief which just as much requires explanation? Doubtless there is here a wide gap in the argument. I hope eventually to fill it up. Here, out of many experiences which conspire to generate this belief, I can but briefly indicate the leading ones: 1. It is not impossible that his shadow, following him everywhere, and moving as he moves, may have some small share in giving to the savage a vague idea of his duality. It needs but to watch a child's interest in the movements of its shadow, and to remember that at first a shadow cannot be interpreted as a negation of light, but is looked upon as an entity, to perceive that the savage may very possibly consider it as a specific something which forms part of him. 2. A much more decided suggestion of the same kind is likely to result from the reflection of his face and figure in water: imitating him as it does in his form, colours, motions, grimaces. When we remember that not unfrequently a savage objects to have his portrait taken, because he thinks whoever carries away a representation of him carries away some part of his being, we see how probable it is that he thinks his double in the water is a reality in some way belonging to him. 3. Echoes must greatly tend to confirm the idea of duality otherwise arrived at. Incapable as he is of understanding their natural origin, the primitive man necessarily ascribes them to living beings—beings who mock him and elude his search. 4. The suggestions resulting from these and other physical phenomena are, however, secondary in importance. The root of this belief in another self lies in the experience of dreams. The distinction so easily made by us between our life in dreams and our real life, is one which the savage recognizes in but a vague way; and he cannot express even that distinction which he perceives. When he awakes, and to those who have seen him lying quietly asleep, describes where he has been, and what he has done, his rude language fails to state the difference between seeing and dreaming that he saw, doing and dreaming that he did. From this inadequacy of his

language it not only results that he cannot truly represent this difference to others, but also that he cannot truly represent it to himself. Hence, in the absence of an alternative interpretation, his belief, and that of those to whom he tells his adventures, is that his other self has been away, and came back when he awoke. And this belief, which we find among various existing savage tribes, we equally find in the traditions of the early civilized races. 5. The conception of another self capable of going away and returning, receives what to the savage must seem conclusive verifications from the abnormal suspensions of consciousness, and derangements of consciousness, that occasionally occur in members of his tribe. One who has fainted, and cannot be immediately brought back to himself (note the significance of our own phrases "returning to himself," etc.) as a sleeper can, shows him a state in which the other self has been away for a time beyond recall. Still more is this prolonged absence of the other self shown him in cases of apoplexy, catalepsy, and other forms of suspended animation. Here for hours the other self persists in remaining away, and on returning refuses to say where he has been. Further verification is afforded by every epileptic subject, into whose body, during the absence of the other self, some enemy has entered; for how else does it happen that the other self, on returning, denies all knowledge of what his body has been doing? And this supposition that the body has been "possessed" by some other being, is confirmed by the phenomena of somnambulism and insanity. 6. What, then, is the interpretation inevitably put upon death? The other self has habitually returned after sleep, which simulates death. It has returned, too, after fainting, which simulates death much more. It has even returned after the rigid state of catalepsy, which simulates death very greatly. Will it not return also after this still more prolonged quiescence and rigidity? Clearly it is quite possible—quite probable even. The dead man's other self is gone away for a long time, but it still exists somewhere, far or near, and may at any moment come back to do all he said he would do. Hence the various burial-rites—the placing of weapons and valuables along with the body, the daily bringing of food to it, etc. I hope hereafter to show that, with such knowledge of the facts as he has, this interpretation is the most reasonable the savage can arrive at. Let me here, however, by way of showing how clearly the facts bear out this view, give one illustration out of many. "The ceremonies with which they [the Veddahs] invoke them [the shades of the dead] are few as they are simple. The most history, or else express an observed community of character with some well-known object. Such a genesis of individual names, before surnames have arisen, is inevitable; and how easily it arises we shall see on remembering that it still goes on in its original form, even when no longer needful. I do not refer only to the significant fact that in some parts of England, as in the nail-making districts, nicknames are general, and surnames little recognized; but I refer to a common usage among both children and adults. The rude man is apt to be known as "a bear;" a sly fellow, as "an old fox;" a hypocrite, as "the crocodile." Names of plants, too, are used; as when the red-haired boy is called "carrots" by his school-fellows. Nor do we lack nicknames derived from inorganic objects and agents: instance that given by Mr. Carlyle to the elder Sterling—"Captain Whirlwind." Now, in the earliest savage state, this metaphorical naming will in most cases commence afresh in each generation—must do so, indeed, until surnames of some kind have been established. I say in most cases, because there will occur exceptions in the cases of men who have distinguished themselves. If "the Wolf,"

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common is the following. An arrow is fixed upright in the ground, and the Veddah dances slowly round it, chanting this invocation, which is almost musical in its rhythm:

"Mâ miya, mâ miy, mâ deyâ,
Topang koyihetti mittigan yandâh?"
"My departed one, my departed one, my God!
Where art thou wandering?"

"This invocation appears to be used on all occasions when the intervention of the guardian spirits is required, in sickness, preparatory to hunting, etc. Sometimes, in the latter case, a portion of the flesh of the game is promised as a votive offering, in the event of the chase being successful; and they believe that the spirits will appear to them in dreams and tell them where to hunt. Sometimes they cook food and place it in the dry bed of a river, or some other secluded spot, and then call on their deceased ancestors by name. 'Come and partake of this! Give us maintenance as you did when living! Come, wheresoever you may be; on a tree, on a rock, in the forest, come!' And they dance round the food, half chanting, half shouting, the invocation."—Bailey, in *Transactions of the Ethnological Society*, London, N. S., ii., p. 301-2.

proving famous in fight, becomes a terror to neighbouring tribes, and a dominant man in his own, his sons, proud of their parentage, will not let fall the fact that they descended from "the Wolf"; nor will this fact be forgotten by the rest of the tribe who hold "the Wolf" in awe, and see reason to dread his sons. In proportion to the power and celebrity of "the Wolf" will this pride and this fear conspire to maintain among his grandchildren and great-grandchildren, as well as among those over whom they dominate, the remembrance of the fact that their ancestor was "the Wolf". And if, as will occasionally happen, this dominant family becomes the root of a new tribe, the members of this tribe will become known to themselves and others as "the Wolves".

We need not rest satisfied with the inference that this inheritance of nicknames *will* take place. There is proof that it *does* take place. As nicknaming after animals, plants, and other objects, still goes on among ourselves, so among ourselves does there go on the descent of nicknames. An instance has come under my own notice on an estate in the West Highlands, belonging to some friends with whom I frequently have the pleasure of spending a few weeks in the autumn. "Take a young Croshek," has more than once been the reply of my host to the inquiry, who should go with me, when I was setting out salmon-fishing. The elder Croshek I knew well; and supposed that this name, borne by him and by all belonging to him, was the family surname. Years passed before I learned that the real surname was Cameron; that the father was called Croshek, after the name of his cottage, to distinguish him from other Camerons employed about the premises; and that his children had come to be similarly distinguished. Though here, as very generally in Scotland, the nickname was derived from the place of residence, yet had it been derived from an animal, the process would have been the same: inheritance of it would have occurred just as naturally. Not even for this small link in the argument, however, need we depend on inference. There is fact to bear us out. Mr. Bates, in his *Naturalist on the River Amazons* (2d ed., p. 376), describing three half-castes who accompanied him on a hunting trip, says—"Two of them were brothers, namely, João (John) and Zephyrino Jabutí: Jabutí, or tortoise, being a nickname which their father had earned for his slow gait, and which, as is usual in this country, had descended as the surname of the family." Let me add the statement made by Mr. Wallace respecting this same region, that "one of the tribes on the river Isánna is called 'Jurupari' (Devils). Another is called 'Ducks;' a third, 'Stars;' a fourth, 'Mandiocca.'" Putting these two statements together, can there be any doubt about the genesis of these tribal names? Let "the Tortoise" become sufficiently distinguished (not necessarily by superiority—great inferiority may occasionally suffice) and the tradition of descent from him, preserved by his descendants themselves if he was superior, and by their contemptuous neighbours if he was inferior, may become a tribal name.*

"But this," it will be said, "does not amount to an explanation of animal-worship." True: a third factor remains to be specified. Given a belief in the still-existing other self of the deceased ancestor, who must be propitiated; given this survival of his metaphorical name among his grandchildren, great-grandchildren, etc.; and the further requisite is that the distinction between metaphor and reality shall be forgotten. Let tradition fail to keep clearly in view the fact that the ancestor was a man called "the Wolf"—let him be habitually spoken of as "the Wolf", just as when alive; and the natural mistake of taking the name literally will bring with it, firstly, a belief in descent from an actual wolf, and, secondly, a treatment of the wolf in a manner likely to propitiate him—a manner appropriate to one who may be the other self of the dead ancestor, or one of the kindred, and therefore a friend.

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* Since the foregoing pages were written, my attention has been drawn by Sir John Lubbock to a passage in the appendix to the second edition of *Prehistoric Times*, in which he has indicated this derivation of tribal names. He says: "In endeavouring to account for the worship of animals, we must remember that names are very frequently taken from them. The children and followers of a man called the Bear or the Lion would make that a tribal name. Hence the animal itself would be first respected, at last worshipped." Of the genesis of this worship, however, Sir John Lubbock does not give any specific explanation. Apparently he inclines to the belief, tacitly adopted also by Mr. McLennan, that animal-worship is derived from an original Fetichism, of which it is a more developed form. As will shortly be seen, I take a different view of its origin.

That a misunderstanding of this kind is likely to grow up, becomes obvious when we bear in mind the great indefiniteness of primitive language. As Prof. Max Müller says, respecting certain misinterpretations of an opposite kind: "These metaphors . . . . would become mere names handed down in the conversation of a family, understood perhaps by the grandfather, familiar to the father, but strange to the son, and misunderstood by the grandson." We have ample reason, then, for supposing such misinterpretations. Nay, we may go further. We are justified in saying that they are certain to occur. For undeveloped languages contain no words capable of indicating the distinction to be kept in view. In the tongues of existing inferior races, only concrete objects and acts are expressible. The Australians have a name for each kind of tree, but no name for tree irrespective of kind. And though some witnesses allege that their vocabulary is not absolutely destitute of generic names, its extreme poverty in such is unquestionable. Similarly with the Tasmanians. Dr. Milligan says they "had acquired very limited powers of abstraction or generalization. They possessed no words representing abstract ideas; for each variety of gum-tree and wattle-tree, etc., etc., they had a name, but they had no equivalent for the expression, 'a tree;' neither could they express abstract qualities, such as hard, soft, warm, cold, long, short, round, etc.; for 'hard,' they would say 'like stone;' for 'tall,' they would say 'long legs,' etc.; and for 'round,' they said 'like a ball,' 'like the moon,' and so on, usually suiting the action to the word, and confirming, by some sign, the meaning to be understood."* Now, even making allowance for over-statement here (which seems needful, since the word "long," said to be inexpressible in the abstract, subsequently occurs as qualifying a concrete in the expression, "long legs"), it is manifest that so imperfect a language must fail to convey the idea of a name, as something separate from a thing; and that still less can it be capable of indicating the act of naming. Familiar use of such partially-abstract words as are applicable to all objects of a class, is needful before there can be reached the conception of a name—a word symbolizing the symbolic character of other words; and the conception of a name, with its answering abstract term, must be long current before the verb to name can arise. Hence, men with speech so rude, cannot transmit the tradition of an ancestor named "the Wolf", as distinguished from the actual wolf. The children and grandchildren who saw him will not be led into error; but in later generations, descent from "the Wolf" will inevitably come to mean descent from the animal known by that name. And the ideas and sentiments which, as above shown, naturally grow up round the belief that the dead parents and grandparents are still alive, and ready, if propitiated, to befriend their descendants, will be extended to the wolf species.

Before passing to other developments of this general view, let me point out how not simply animal-worship is thus accounted for, but also the conception, so variously illustrated in ancient legends, that animals are capable of displaying human powers of speech and thought and action. Mythologies are full of stories of beasts and birds and fishes that have played intelligent parts in human affairs—

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* *Proceedings of the Royal Society of Tasmania*, iii., p. 280-81.

creatures that have befriended particular persons by giving them information, by guiding them, by yielding them help; or else that have deceived them, verbally or otherwise. Evidently all these traditions, as well as those about abductions of women by animals and fostering of children by them, fall naturally into their places as results of the habitual misinterpretation I have described.

The probability of the hypothesis will appear still greater when we observe how readily it applies to the worship of other orders of objects. Belief in actual descent from an animal, strange as we may think it, is one by no means incongrūous with the unanalyzed experiences of the savage; for there come under his notice many metamorphoses, vegetal and animal, which are apparently of like character. But how could he possibly arrive at so grotesque a conception as that the progenitor of his tribe was the sun, or the moon, or a particular star? No observation of surrounding phenomena affords the slightest suggestion of any such possibility. But by the inheritance of nicknames that are eventually mistaken for the names of the objects from which they were derived, the belief readily arises—is sure to arise. That the names of heavenly bodies will furnish metaphorical names to the uncivilized, is manifest. Do we not ourselves call a distinguished singer or actor a star? And have we not in poems numerous comparisons of men and women to the sun and moon; as in *Love's Labour's Lost,* where the princess is called "a gracious moon," and as in *Henry VII.*, where we read—"Those suns of glory, those two lights of men?" Clearly, primitive peoples will be not unlikely thus to speak of the chief hero of a successful battle. When we remember how the arrival of a triumphant warrior must affect the feelings of his tribe, dissipating clouds of anxiety and brightening all faces with joy, we shall see that the comparison of him to the sun is quite natural; and in early speech this comparison can be made only by calling him the sun. As before, then, it will happen that, through a confounding of the metaphorical name with the actual name, his progeny, after a few generations, will be regarded by themselves and others as descendants of the sun. And, as a consequence, partly of actual inheritance of the ancestral character, and partly of maintenance of the traditions respecting the ancestor's achievements, it will also naturally happen that the solar race will be considered a superior race, as we find it habitually is.

The origin of other totems, equally strange, if not even stranger, is similarly accounted for, though otherwise unaccountable. One of the New-Zealand chiefs claimed as his progenitor the neighbouring great mountain, Tongariro. This seemingly-whimsical belief becomes intelligible when we observe how easily it may have arisen from a nickname. Do we not ourselves sometimes speak figuratively of a tall, fat man as a mountain of flesh? And, among a people prone to speak in still more concrete terms, would it not happen that a chief, remarkable for his great bulk, would be nicknamed after the highest mountain within sight, because he towered above other men as this did above surrounding hills? Such an occurrence is not simply possible, but probable. And, if so, the confusion of metaphor with fact would originate this surprising genealogy. A notion perhaps yet more grotesque, thus receives a satisfactory interpretation. What could have put it into the imagination of any one that he was descended from the dawn? Given the extremest credulity, joined with the wildest fancy, it would still seem requisite that the ancestor should be conceived as an entity; and the dawn is entirely without that definiteness and comparative constancy which enter into the conception of an entity. But when we remember that "the Dawn" is a natural complimentary name for a beautiful girl opening into womanhood, the genesis of the idea becomes, on the above hypothesis, quite obvious.*

Another indirect verification is that we thus get a clear conception of Fetichism in general. Under the fetichistic mode of thought, surrounding objects and agents are regarded as having powers more or less definitely personal in their natures; and the current interpretation is, that human intelligence, in its early stages, is obliged to conceive of their powers under this form. I have myself hitherto accepted this interpretation; though always with a sense of dissatisfaction. This dissatisfaction was, I think, well grounded. The theory is scarcely a theory properly so-called; but rather, a restatement in other words. Uncivilized men *do* habitually form anthropomorphic conceptions of surrounding things; and this observed general fact is transformed into the theory that at first they *must* so conceive them—a theory for which the psychological justification attempted, seems to me inadequate. From our present stand-point, it becomes manifest that Fetichism is not primary but secondary. What has been said above almost of itself shows this. Let us, however, follow out the steps of its genesis. Respecting the Tasmanians, Dr. Milligan says:—"The names of men and women were taken from natural objects and occurrences around, as, for instance, a kangaroo, a gum tree, snow, hail, thunder, the wind," flowers in blossom, etc. Surrounding objects, then, giving origin to names of persons, and being, in the way shown, eventually mistaken for the actual progenitors of those who descend from persons nicknamed after them, it results that these surrounding objects come to be regarded as in some manner possessed of personalities like the human. He whose family tradition is that his ancestor was "the Crab," will conceive the crab as having a disguised inner power like his own; an alleged descent from "the Palm-tree" will entail belief in some kind of consciousness dwelling in the palm-tree. Hence, in proportion as the animals, plants, and inanimate objects or agents that originate names of persons, become numerous (which they will do in proportion as a tribe becomes large and the number of persons to be distinguished from one another increases), multitudinous things around will acquire imaginary personalities. And so it will happen that, as Mr. McLennan says of the Feejeeans, "Vegetables and stones, nay, even tools and weapons, pots and canoes, have souls that are immortal, and that, like the souls of men, pass on at last to Mbulu, the abode of departed spirits." Setting out, then, with a belief in the still-living other self of the dead ancestor, the alleged general cause of misapprehension affords us an intelligible origin of the fetichistic conception; and we are enabled to see how it tends to become a general, if not a universal, conception.

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* I have since found, however, that the name Dawn, which occurs in various places, seems more frequently a birth-name, given because the birth took place at dawn.

Other apparently inexplicable phenomena are at the same time divested of their strangeness. I refer to the beliefs in, and worship of, compound monsters—impossible hybrid animals, and forms that are half human, half brutal. The theory of a primordial Fetichism, supposing it otherwise adequate, yields no feasible solutions of these. Grant the alleged original tendency to think of all natural agencies as in some way personal. Grant, too, that hence may arise a worship of animals, plants, and even inanimate bodies. Still the obvious implication is that the worship so derived will be limited to things that are, or have been, perceived. Why should this mode of thought lead the savage to imagine a combination of bird and mammal; and not only to imagine it, but to worship it as a god? If even we admit that some illusion may have suggested the belief in a creature half man, half fish, we cannot thus explain the prevalence among Eastern races of idols representing bird-headed men, and men having their legs replaced by the legs of a cock, and men with the heads of elephants.

Carrying with us the inferences above drawn, however, it is a corollary that ideas and practices of these kinds will arise. When tradition preserves both lines of ancestry—when a chief, nicknamed "the Wolf", carries away from an adjacent tribe a wife who is remembered either under the animal name of her tribe, or as a woman; it will happen that if a son distinguishes himself, the remembrance of him among his descendants will be that he was born of a wolf and some other animal, or of a wolf and a woman. Misinterpretation, arising in the way described from defects of language, will entail belief in a creature uniting the attributes of the two; and if the tribe grows into a society, representations of such a creature will become objects of worship. One of the cases cited by Mr. McLennan may here be repeated in illustration. "The story of the origin of the Dikokamenni Kirgheez," they say, "from a red greyhound and a certain queen and her forty handmaidens, is of ancient date." Now, if "the red greyhound" was the nickname of a man extremely swift of foot (celebrated runners have been nicknamed "greyhound" among ourselves), a story of this kind would naturally arise; and if the metaphorical name was mistaken for the actual name, there might result, as the idol of the race, a compound form appropriate to the story. We need not be surprised, then, at finding among the Egyptians the goddess Pasht represented as a woman with a lion's head, and the god Har-hat as a man with the head of a hawk. The Babylonian gods—one having the form of a man with an eagle's tail, and another uniting a human bust to a fish's body—no longer appear such unaccountable conceptions. We get feasible explanations, too, of sculptures representing sphinxes, winged human-headed bulls, etc.; as well as of the stories about centaurs, satyrs, and the rest.

Ancient myths in general thus acquire meanings considerably different from those ascribed to them by comparative mythologists. Though these last may be in part correct, yet if the foregoing argument is valid, they can scarcely be correct in their main outlines. Indeed, if we read the facts the other way upward, regarding as secondary or additional, the elements that are said to be primary, while we regard as primary, certain elements which are considered as accretions of later times, we shall, I think, be nearer the truth.

The current theory of the myth is that it has grown out of the habit of symbolizing natural agents and processes, in terms of human personalities and actions. Now, it may in the first place be remarked that, though symbolization of this kind is common among civilized races, it is not common among races that are the most uncivilized. By existing savages, surrounding objects, motions, and changes, are habitually used to convey ideas respecting human transactions. It needs but to read the speech of an Indian chief to see that just as primitive men name one another metaphorically after surrounding objects, so do they metaphorically describe one another's doings as though they were the doings of natural objects. But assuming a contrary habit of thought to be the dominant one, ancient myths are explained as results of the primitive tendency to symbolize inanimate things and their changes, by human beings and their doings.

A kindred difficulty must be added. The change of verbal meaning from which the myth is said to arise, is a change opposite in kind to that which prevails in the earlier stages of linguistic development. It implies a derivation of the concrete from the abstract; whereas at first abstracts are derived only from concretes: the concrete of abstracts being a subsequent process. In the words of Prof. Max Müller, there are "dialects spoken at the present day which have no abstract nouns, and the more we go back in the history of languages, the smaller we find the number of these useful expressions" (*Chips*, vol. ii., p. 54); or, as he says more recently—"Ancient words and ancient thoughts, for both go together, have not yet arrived at that stage of abstraction in which, for instance, active powers, whether natural or supernatural, can be represented in any but a personal and more or less human form." (*Fraser's Magazine*, April, 1870.) Here the concrete is represented as original, and the abstract as derivative. Immediately afterward, however, Prof. Max Müller, having given as examples of abstract nouns, "day and night, spring and winter, dawn and twilight, storm and thunder," goes on to argue that, "as long as people thought in language, it was simply impossible to speak of morning or evening, of spring and winter, without giving to these conceptions something of an individual, active, sexual, and at last, personal character." (*Chips*, vol. ii., p. 55.) Here the concrete is derived from the abstract—the personal conception is represented as coming *after* the impersonal conception; and through such transformation of the impersonal into the personal, Prof. Max Müller considers ancient myths to have arisen. How are these propositions reconcilable? One of two things must be said:—If originally there were none of these abstract nouns, then the earliest statements respecting the daily course of Nature were made in concrete terms—the personal elements of the myth were the primitive elements, and the impersonal expressions which are their equivalents came later. If this is not admitted, then it must be held that, until after there arose these abstract nouns, there were no current statements at all respecting these most conspicuous objects and changes which the heavens and the earth present; and that the abstract nouns having been somehow formed, and rightly formed, and used without personal meanings, afterward became personalized —a process the reverse of that which characterizes early linguistic progress.

No such contradictions occur if we interpret myths after the manner that has been indicated. Nay, besides escaping contradictions, we meet with unexpected solutions. The moment we try it, the key unlocks for us with ease what seems a quite inexplicable fact, which the current hypothesis takes as one of its postulates. Speaking of such words as sky and earth, dew and rain, rivers and mountains, as well as of the abstract nouns above named, Prof. Max Müller says—"Now in ancient languages every one of these words had necessarily a termination expressive of gender, and this naturally produced in the mind the corresponding idea of sex, so that these names received not only an individual, but a sexual character. There was no substantive which was not either masculine or feminine; neuters being of later growth, and distinguishable chiefly in the nominative." (*Chips*, vol. ii., p. 55.) And this alleged necessity for a masculine or feminine implication is assigned as a part of the reason why these abstract nouns and collective nouns became personalized. But should not a true theory of these first steps in the evolution of thought and language show us how it happened that men acquired the seemingly-strange habit of so framing their words for sky, earth, dew, rain, etc., as to make them indicative of sex? Or, at any rate, must it not be admitted that an interpretation which, instead of assuming this habit to be "necessary," shows us how it results, thereby acquires an additional claim to acceptance? The interpretation I have indicated does this. If men and women are habitually nicknamed, and if defects of language lead their descendants to regard themselves as descendants of the things from which the names were taken, then masculine or feminine genders will be ascribed to these things according as the ancestors named after them were men or women. If a beautiful maiden known metaphorically as "the Dawn," afterwards becomes the mother of some distinguished chief called "the North Wind," it will result that when, in course of time, the two have been mistaken for the actual dawn and the actual north wind, these will, by implication, be respectively considered as male and female.

Looking, now, at the ancient myths in general, their seemingly most inexplicable trait is the habitual combination of alleged human ancestry and adventures, with the possession of personalities otherwise figuring in the heavens and on the earth, with totally non-human attributes. This enormous incongruity, not the exception but the rule, the current theory fails to explain. Suppose it to be granted that the great terrestrial and celestial objects and agents naturally become personalized; it does not follow that each of them shall have a specific human biography. To say of some star that he was the son of this king or that hero, was born in a particular place, and when grown up carried off the wife of a neighbouring chief, is a gratuitous multiplication of incongruities already sufficiently great; and is not accounted for by the alleged necessary personalization of abstract and collective nouns. As looked at from our present standpoint, however, such traditions become quite natural—nay, it is clear that they will necessarily arise. When a nickname has become a tribal name, it thereby ceases to be individually distinctive; and, as already said, the process of nicknaming inevitably continues. It commences afresh with each child; and the nickname of each child is both an individual name and a potential tribal name, which may become an actual tribal name if the individual is sufficiently celebrated. Usually, then, there is a double set of distinctions; under one of which the individual is known by his ancestral name, and under the other of which he is known by a name suggestive of something peculiar to himself: just as we have seen happens among the Scotch clans. Consider, now, what will result when language has reached a stage of development such that it can convey the notion of naming, and is able, therefore, to preserve traditions of human ancestry. It will result that the individual will be known both as the son of such and such a man by a mother whose name was so and so, and also as "the Crab", or "the Bear", or "the Whirlwind"—supposing one of these to be his nickname. Such joint use of nicknames and proper names occurs in every school. Now, clearly, in advancing from the early state in which ancestors become identified with the objects they are nicknamed after, to the state in which there are proper names that have lost their metaphorical meanings, there must be passed through a state in which proper names, partially settled only, may or may not be preserved, and in which the new nicknames are still liable to be mistaken for actual names. Under such conditions there will arise (especially in the case of a distinguished man) this seemingly-impossible combination of human parentage with the possession of the non-human, or superhuman, attributes of the thing which gave the nickname. Another anomaly simultaneously disappears. The warrior may have, and often will have, a variety of complimentary nicknames—"the powerful one," "the destroyer," etc. Supposing his leading nickname has been "the Sun"; then when he comes to be identified by tradition with the sun, it will happen that the sun will acquire his alternative descriptive titles—the swift one, the lion, the wolf—titles not obviously appropriate to the sun, but quite appropriate to the warrior. Then there comes, too, an explanation of the remaining trait of such myths. When this identification of conspicuous persons, male and female, with conspicuous natural agents, has become settled, there will in due course arise interpretations of the actions of these agents in anthropomorphic terms. Suppose, for instance, that Endymion and Selene, metaphorically named, the one after the setting sun, the other after the moon, have had their human individualities merged in those of the sun and moon, through misinterpretation of metaphors; what will happen? The legend of their loves having to be reconciled with their celestial appearances and motions, these will be spoken of as results of feeling and will; so that when the sun is going down in the west, while the moon in mid-heaven is following him, the fact will be expressed by saying: "Selene loves and watches Endymion." Thus we obtain a consistent explanation of the myth without distorting it; and without assuming that it contains gratuitous fictions. We are enabled to accept the biographical part of it, if not as literal fact, still as having had fact for its root. We are helped to see how, by an inevitable misinterpretation, there grew out of a more or less true tradition, this strange identification of its personages, with objects and powers totally non-human in their aspects. And then we are shown how, from the attempt to reconcile in thought these contradictory elements of the myth, there arose the habit of ascribing the actions of these non-human things to human motives.

One further verification may be drawn from facts which are obstacles to the converse hypothesis. These objects and powers, celestial and terrestrial, which force themselves most on men's attention, have some of them several proper names, identified with those of different individuals, born at different places, and having different sets of adventures. Thus we have the sun variously known as Apollo, Endymion, Helios, Tithonos, etc.—personages having irreconcilable genealogies. Such anomalies Prof. Max Müller apparently ascribes to the untrustworthiness of traditions, which are "careless about contradictions, or ready to solve them sometimes by the most atrocious expedients." (*Chips*, vol. ii., p. 84.) But if the evolution of the myth has been that above indicated, there exists no anomalies to be got rid of: these diverse genealogies become parts of the evidence. For we have abundant proof that the same objects furnish metaphorical names of men in different tribes. There are Duck tribes in Australia, in

South America, in North America. The eagle is still a totem among the North Americans, as Mr. McLennan shows reason to conclude that it was among the Egyptians, among the Jews, and among the Romans. Obviously, for reasons already assigned, it naturally happened in the early stages of the ancient races, that complimentary comparisons of their heroes to the Sun were frequently made. What resulted? The Sun having furnished names for sundry chiefs and early founders of tribes, and local traditions having severally identified them with the Sun, these tribes, when they grew, spread, conquered, or came otherwise into partial union, originated a combined mythology, which necessarily contained conflicting stories about the Sun-god, as about its other leading personages. If the North-American tribes, among several of which there are traditions of a Sun-god, had developed a combined civilization, there would similarly have arisen among them a mythology which ascribed to the Sun several different proper names and genealogies.

Let me briefly set down the leading characters of this hypothesis which give it probability.

True interpretations of all the natural processes, organic and inorganic, that have gone on in past times, habitually trace them to causes still in action. It is thus in Geology; it is thus in Biology; it is thus in Philology. Here we find this characteristic repeated. Nicknaming, the inheritance of nicknames, and to some extent, the misinterpretation of nicknames, go on among us still; and were surnames absent, language imperfect, and knowledge as rudimentary as of old, it is tolerably manifest that results would arise like those we have contemplated.

A further characteristic of a true cause is that it accounts not only for the particular group of phenomena to be interpreted, but also for other groups. The cause here alleged does this. It equally well explains the worship of animals, of plants, of mountains, of winds, of celestial bodies, and even of appearances too vague to be considered entities. It gives us an intelligible genesis of fetichistic conceptions in general. It furnishes us with a reason for the practice, otherwise so unaccountable, of moulding the words applied to inanimate objects in such ways as to imply masculine and feminine genders. It shows us how there naturally arose the worship of compound animals, and of monsters half man, half brute. And it shows us why the worship of purely anthropomorphic deities came later, when language had so far developed that it could preserve in tradition the distinction between proper names and nicknames.

A further verification of this view is, that it conforms to the general law of evolution: showing us how, out of one simple, vague, aboriginal form of belief, there have arisen, by continuous differentiations, the many heterogeneous forms of belief which have existed and do exist. The desire to propitiate the other self of the dead ancestor, displayed among savage tribes, dominantly manifested by the early historic races, by the Peruvians and Mexicans, by the Chinese at the present time, and to a considerable degree by ourselves (for what else is the wish to do that which a lately-deceased parent was known to have desired?) has been the universal first form of religious belief; and from it have grown up the many divergent beliefs which have been referred to.

Let me add, as a further reason for adopting this view, that it immensely diminishes the apparently-great contrast between early modes of thought and our own mode of thought. Doubtless the aboriginal man differs considerably from us, both in intellect and feeling. But such an interpretation of the facts as helps us to bridge over the gap, derives additional likelihood from doing this. The hypothesis I have sketched out enables us to see that primitive ideas are not so gratuitously absurd as we suppose, and also enables us to rehabilitate the ancient myth with far less distortion than at first sight appears possible.

These views I hope to develop in the first part of *The Principles of Sociology.* The large mass of evidence which I shall be able to give in support of the hypothesis, joined with the solutions it will be shown to yield of many minor problems which I have passed over, will, I think, then give to it a still greater probability than it seems now to have.

# MORALS AND MORAL SENTIMENTS.

[*First published in* The Fortnightly Review *for April*, 1871.]

If a writer who discusses unsettled questions takes up every gauntlet thrown down to him, polemical writing will absorb much of his energy. Having a power of work which unfortunately does not suffice for executing with anything like due rapidity the task I have undertaken, I have made it a policy to avoid controversy as much as possible, even at the cost of being seriously misunderstood. Hence it resulted that when in *Macmillan's Magazine*, for July, 1869, Mr. Richard Hutton published, under the title "A Questionable Parentage for Morals," a criticism on a doctrine of mine, I decided to let his misrepresentations pass unnoticed until, in the course of my work, I arrived at the stage where, by a full exposition of this doctrine, they would be set aside. It did not occur to me that, in the meantime, these erroneous statements, accepted as true statements, would be repeated by other writers, and my views commented upon as untenable. This, however, has happened. In more periodicals than one, I have seen it asserted that Mr. Hutton has effectually disposed of my hypothesis. Supposing that this hypothesis has been rightly expressed by Mr. Hutton, Sir John Lubbock, in his *Origin of Civilisation*, &c., has been led to express a partial dissent; which I think he would not have expressed had my own exposition been before him. Mr. Mivart, too, in his recent *Genesis of Species*, has been similarly betrayed into misapprehensions. And now Sir Alexander Grant, following the same lead, has conveyed to the readers of the *Fortnightly Review* another of these conceptions, which is but very partially true. Thus I find myself compelled to say as much as will serve to prevent further spread of the mischief.

If a general doctrine concerning a highly-involved class of phenomena could be adequately presented in a single paragraph of a letter, the writing of books would be superfluous. In the brief exposition of certain ethical doctrines held by me, which is given in Professor Bain's *Mental and Moral Science*, it is stated that they are—

> "as yet, nowhere fully expressed. They form part of the more general doctrine of Evolution which he is engaged in working out; and they are at present to be gathered only from scattered passages. It is true that, in his first work, *Social Statics*, he presented what he then regarded as a tolerably complete view of one division of Morals. But without abandoning this view, he now regards it as inadequate—more especially in respect of its basis."

Mr. Hutton, however, taking the bare enunciation of one part of this basis, deals with it critically; and, in the absence of any exposition by me, sets forth what he supposes to be my grounds for it, and proceeds to show that they are unsatisfactory.

If, in his anxiety to suppress what he doubtless regards as a pernicious doctrine, Mr. Hutton could not wait until I had explained myself, it might have been expected that he would use whatever information was to be had concerning it. So far from seeking out such information, however, he has, in a way for which I cannot account, ignored the information immediately before him.

The title which Mr. Hutton has chosen for his criticism is, "A Questionable Parentage for Morals." Now he has ample means of knowing that I allege a primary basis of

Morals, quite independent of that which he describes and rejects. I do not refer merely to the fact that having, when he reviewed *Social Statics*,* expressed his very decided dissent from this primary basis, he must have been aware that I alleged it; for he may say that in the many years which have since elapsed he had forgotten all about it. But I refer to the distinct enunciation of this primary basis in that letter to Mr. Mill from which he quotes. In a preceding paragraph of the letter, I have explained that, while I accept utilitarianism in the abstract, I do not accept that current utilitarianism which recognizes for the guidance of conduct nothing beyond empirical generalizations; and I have contended that—

> "Morality, properly so-called—the science of right conduct—has for its object to determine *how* and *why* certain modes of conduct are detrimental, and certain other modes beneficial. These good and bad results cannot be accidental, but must be necessary consequences of the constitution of things; and I conceive it to be the business of Moral Science to deduce, from the laws of life and the conditions of existence, what kinds of action necessarily tend to produce happiness, and what kinds to produce unhappiness. Having done this, its deductions are to be recognised as laws of conduct; and are to be conformed to irrespective of a direct estimation of happiness or misery."

Nor is this the only enunciation of what I conceive to be the primary basis of morals, contained in this same letter. A subsequent paragraph separated by four lines only from that which Mr. Hutton extracts, commences thus:—

> "Progressing civilization, which is of necessity a succession of compromises between old and new, requires a perpetual re-adjustment of the compromise between the ideal and the practicable in social arrangements: to which end, both elements of the compromise must be kept in view. If it is true that pure rectitude prescribes a system of things far too good for men as they are, it is not less true that mere expediency does not of itself tend to establish a system of things any better than that which exists. While absolute morality owes to expediency the checks which prevent it from rushing into Utopian absurdities, expediency is indebted to absolute morality for all stimulus to improvement. Granted that we are chiefly interested in ascertaining what is *relatively right*, it still follows that we

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* See *Prospective Review* for January, 1852.

must first consider what is *absolutely right;* since the one conception presupposes the other."

I do not see how there could well be a more emphatic assertion that there exists a primary basis of morals independent of, and in a sense antecedent to, that which is furnished by experiences of utility; and consequently, independent of, and, in a sense antecedent to, those moral sentiments which I conceive to be generated by such experiences. Yet no one could gather from Mr. Hutton's article that I assert this; or would even find reasons for a faint suspicion that I do so. From the reference made to my further views, he would infer my acceptance of that empirical utilitarianism which I have expressly repudiated. And the title which Mr. Hutton gives to his paper clearly asserts, by implication, that I recognize no "parentage for morals" beyond that of the accumulation and organization of the effects of experience. I cannot believe that Mr. Hutton intended to convey this erroneous impression. He was, I suppose, too much absorbed in contemplating the proposition he combats to observe, or, at least, to attach any weight to, the propositions which accompany it. But I am sorry he did not perceive the mischief he was likely to do me by spreading this one-sided statement.

I pass now to the particular question at issue—not the "parentage for morals," but the parentage of moral sentiments. In describing my view on this more special doctrine, Mr. Hutton has similarly, I regret to say, neglected the data which would have helped him to draw an approximately true outline of it. It cannot well be that the existence of such data was unknown to him. They are contained in the *Principles of Psychology;* and Mr. Hutton reviewed that work when it was first published.* In a chapter on the Feelings, which occurs near the end of it, there is sketched out a process of evolution by no means like that which Mr. Hutton indicates; and had he turned to that chapter he would have seen that his description of the genesis of moral sentiments out of organized experiences is not such a one as I should have given. Let me quote a passage from that chapter.

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* His criticism will be found in the *National Review* for January, 1856, under the title "Atheism."

> "Not only are those emotions which form the immediate stimuli to actions, thus explicable; but the like explanation applies to the emotions that leave the subject of them comparatively passive: as, for instance, the emotion produced by beautiful scenery. The gradually increasing complexity in the groups of sensations and ideas co-ordinated, ends in the co-ordination of those vast aggregations of them which a grand landscape excites and suggests. The infant taken into the midst of mountains, is totally unaffected by them; but is delighted with the small group of attributes and relations presented in a toy. The child can appreciate, and be pleased with, the more complicated relations of household objects and localities, the garden, the field, and the street. But it is only in youth and mature age, when individual things and small assemblages of them have become familiar and automatically cognizable, that those immense assemblages which landscapes present can be adequately grasped, and the highly aggregated states of consciousness produced by them, experienced. Then, however, the various minor groups of states that have been in earlier days severally produced by trees, by fields, by streams, by cascades, by rocks, by precipices, by mountains, by clouds, are aroused together. Along with the sensations immediately received, there are partially excited the myriads of sensations that have been in times past received from objects such as those presented; further, there are partially excited the various incidental feelings that were experienced on all these countless past occasions; and there are probably also excited certain deeper, but now vague combinations of states, that were organized in the race during barbarous times, when its pleasurable activities were chiefly among the woods and waters. And out of all these excitations, some of them actual but most of them nascent, is composed the emotion which a fine landscape produces in us."

It is, I think, amply manifest that the processes here indicated are not to be taken as intellectual processes—not as processes in which recognized relations between pleasures and their antecedents, or intelligent adaptations of means to ends, form the dominant elements. The state of mind produced by an aggregate of picturesque objects is not one resolvable into propositions. The sentiment does not contain within itself any consciousness of causes and consequences of happiness. The vague recollections of other beautiful scenes and other delightful days which it dimly rouses, are not aroused because of any rational co-ordinations of ideas that have been formed in bygone years. Mr. Hutton, however, assumes that in speaking of the genesis of moral feelings as due to inherited experiences of the pleasures and pains caused by certain modes of conduct, I am speaking of reasoned-out experiences—experiences consciously accumulated and generalized. He overlooks the fact that the genesis of emotions is distinguished from the genesis of ideas in this; that whereas the ideas are composed of elements that are simple, definitely related, and (in the case of general ideas) constantly related, emotions are composed of enormously complex aggregates of elements that are never twice alike, and which stand in relations that are never twice alike. The difference in the resulting modes of consciousness is this:—In the genesis of an idea the successive experiences, be they of sounds, colours, touches, tastes, or be they of the special objects which combine many of these into groups, have so much in common that each, when it occurs, can be definitely thought of as like those which preceded it. But in the genesis of an emotion the successive experiences so far differ that each of them, when it occurs, suggests past experiences which are not specifically similar, but have only a general similarity; and, at the same time, it suggests benefits or evils in past experience which likewise are various in their special natures, though they have a certain community in general nature. Hence it results that the consciousness aroused is a multitudinous, confused consciousness, in which, along with a certain kind of combination among the impressions received from without, there is a vague cloud of ideal combinations akin to them, and a vague mass of ideal feelings of pleasure or pain which were associated with these. We have abundant proof that feelings grow up without reference to recognized causes and consequences, and without the possessor of them being able to say why they have grown up; though analysis, nevertheless, shows that they have been formed out of connected experiences. The familiar fact that a kind of jam which was, during childhood, repeatedly taken after medicine, may become, by simple association of sensations, so nauseous that it cannot be tolerated in after-life, illustrates clearly the way in which repugnances may be established by habitual association of feelings, without any belief in causal connexion; or rather, in spite of the knowledge that there is no causal connexion. Similarly with pleasurable emotions. The cawing of rooks is not in itself an agreeable sound: musically considered, it is very much the contrary. Yet the cawing of rooks usually produces in people feelings of a grateful kind—feelings which most of them suppose to result from the quality of the sound itself. Only the few who are given to self-analysis are aware that the cawing of rooks is agreeable to them because it has been connected with countless of their greatest gratifications—with the gathering of wild flowers in childhood; with Saturday-afternoon excursions in school-boy days; with midsummer holidays in the country, when books were thrown aside and lessons were replaced by games and adventures in the fields; with fresh, sunny mornings in after-years, when a walking excursion was an immense relief from toil. As it is, this sound, though not causally related to all these multitudinous and varied past delights, but only often associated with them, can no more be heard without rousing a dim consciousness of these delights, than the voice of an old friend unexpectedly coming into the house can be heard without suddenly raising a wave of that feeling that has resulted from the pleasures of past companionship. If we are to understand the genesis of emotions, either in the individual or in the race, we must take account of this all-important process. Mr. Hutton, however, apparently overlooking it, and not having reminded himself, by referring to the *Principles of Psychology*, that I insist upon it, represents my hypothesis to be that a certain sentiment results from the consolidation of intellectual conclusions! He speaks of me as believing that "what seems to us now the 'necessary' intuitions and *a priori* assumptions of human nature, are likely to prove, when scientifically analysed, nothing but a similar conglomeration of our ancestors' *best observations and most useful empirical rules.*" He supposes me to think that men having, in past times, come to *see* that truthfulness was useful, "the habit of approving truth-speaking and fidelity to engagements, which was first based on this ground of utility, became so rooted, that the utilitarian ground of it was forgotten, and *we* find ourselves springing to the belief in truth-speaking and fidelity to engagements from an inherited tendency." Similarly throughout, Mr. Hutton has so used the word "utility," and so interpreted it on my behalf, as to make me appear to mean that moral sentiment is formed out of *conscious generalizations* respecting what is beneficial and what detrimental. Were such my hypothesis, his criticisms would be very much to the point; but as such is not my hypothesis, they fall to the ground. The experiences of utility I refer to are those which become registered, not as distinctly recognized connexions between certain kinds of acts and certain kinds of remote results, but those which become registered in the shape of associations between groups of feelings that have often recurred together, though the relation between them has not been consciously generalized—associations the origin of which may be as little perceived as is the origin of the pleasure given by the sounds of a rookery; but which, nevertheless, have arisen in the course of daily converse with things, and serve as incentives or deterrents.

In the paragraph which Mr. Hutton has extracted from my letter to Mr. Mill, I have indicated an analogy between those effects of emotional experiences out of which I believe moral sentiments have been developed, and those effects of intellectual experiences out of which I believe space-intuitions have been developed. Rightly considering that the first of these hypotheses cannot stand if the last is disproved, Mr. Hutton has directed part of his attack against this last. But would it not have been well if he had referred to the *Principles of Psychology*, where this last hypothesis is set forth at length, before criticising it? Would it not have been well to give an abstract of my own description of the process, instead of substituting what he *supposes* my description must be? Any one who turns to the *Principles of Psychology* (first edition, pp. 218-245), and reads the two chapters, "The Perception of Body as presenting Statical Attributes", and "The Perception of Space", will find that Mr. Hutton's account of my view on this matter has given him no notion of the view as it is expressed by me; and will, perhaps, be less inclined to smile than he was when he read Mr. Hutton's account. I cannot here do more than thus imply the invalidity of such part of Mr. Hutton's argument as proceeds upon this incorrect representation. The pages which would be required for properly explaining the doctrine that space-intuitions result from organized experiences may be better used for explaining this analogous doctrine at present before us. This I will now endeavour to do; not indirectly by correcting misapprehensions, but directly by an exposition which shall be as brief as the extremely involved nature of the process allows.

An infant in arms, when old enough to gaze at objects around with some vague recognition, smiles in response to the laughing face and soft caressing voice of its mother. Let there come some one who, with an angry face, speaks to it in loud, harsh tones. The smile disappears, the features contract into an expression of pain, and, beginning to cry, it turns away its head, and makes such movements of escape as are possible. What is the meaning of these facts? Why does not the frown make it smile, and the mother's laugh make it weep? There is but one answer.

Already in its developing brain there is coming into play the structure through which one cluster of visual and auditory impressions excites pleasurable feelings, and the structure through which another cluster of visual and auditory impressions excites painful feelings. The infant knows no more about the relation existing between a ferocious expression of face, and the evils which may follow perception of it, than the young bird just out of its nest knows of the possible pain and death which may be inflicted by a man coming towards it; and as certainly in the one case as in the other, the alarm felt is due to a partially-established nervous structure. Why does this partially-established nervous structure betray its presence thus early in the human being? Simply because, in the past experiences of the human race, smiles and gentle tones in those around have been the habitual accompaniments of pleasurable feelings; while pains of many kinds, immediate and more or less remote, have been continually associated with the impressions received from knit brows, and set teeth, and grating voice. Much deeper down than the history of the human race must we go to find the beginnings of these connexions. The appearances and sounds which excite in the infant a vague dread, indicate danger; and do so because they are the physiological accompaniments of destructive action—some of them common to man and inferior mammals, and consequently understood by inferior mammals, as every puppy shows us. What we call the natural language of anger, is due to a partial contraction of those muscles which actual combat would call into play; and all marks of irritation, down to that passing shade over the brow which accompanies slight annoyance, are incipient stages of these same contractions. Conversely with the natural language of pleasure, and of that state of mind which we call amicable feeling: this, too, has a physiological interpretation.*

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* Hereafter I hope to elucidate at length these phenomena of expression.

Let us pass now from the infant in arms to the children in the nursery. What have the experiences of each been doing in aid of the emotional development we are considering? While its limbs have been growing more agile by exercise, its manipulative skill increasing by practice, its perceptions of objects growing by use quicker, more accurate, more comprehensive; the associations between these two sets of impressions received from those around, and the pleasures and pains received along with them, or after them, have been by frequent repetition made stronger, and their adjustments better. The dim sense of pain and the vague glow of delight which the infant felt, have, in the urchin, severally taken shapes that are more definite. The angry voice of a nursemaid no longer arouses only a formless feeling of dread, but also a specific idea of the slap that may follow. The frown on the face of a bigger brother, along with the primitive, indefinable sense of ill, brings the ideas of ills that are definable as kicks, and cuffs, and pullings of hair, and losses of toys. The faces of parents, looking now sunny, now gloomy, have grown to be respectively associated with multitudinous forms of gratification and multitudinous forms of discomfort or privation. Hence these appearances and sounds, which imply amity or enmity in those around, become symbolic of happiness and misery; so that eventually, perception of the one set or the other can scarcely occur without raising a wave of pleasurable feeling or of painful feeling. The body of this wave is still substantially of the same nature as it was at first; for though in each of these multitudinous experiences a special set of facial and vocal signs has been connected with a special set of pleasures or pains; yet since these pleasures or pains have been immensely varied in their kinds and combinations, and since the signs that preceded them were

For the present, I can refer only to such further indications as are contained in two essays on "The Physiology of Laughter" and "The Origin and Function of Music."

in no two cases quite alike, it results that even to the end the consciousness produced remains as vague as it is voluminous. The thousands of partially-aroused ideas resulting from past experiences are massed together and superposed, so as to form an aggregate in which nothing is distinct, but which has the character of being pleasurable or painful according to the nature of its original components: the chief difference between this developed feeling and the feeling aroused in the infant being, that on bright or dark background forming the body of it, may now be sketched out in thought the particular pleasures or pains which the particular circumstances suggest as likely.

What must be the working of this process under the conditions of aboriginal life? The emotions given to the young savage by the natural language of love and hate in the members of his tribe, gain first a partial definiteness in respect to his intercourse with his family and playmates; and he learns by experience the utility, in so far as his own ends are concerned, of avoiding courses which call from others manifestations of anger, and taking courses which call from them manifestations of pleasure. Not that he consciously generalizes. He does not at that age, probably not at any age, formulate his experiences in the general principle that it is well for him to do things which bring smiles, and to avoid doing things which bring frowns. What happens is that having, in the way shown, inherited this connexion between the perception of anger in others and the feeling of dread, and having discovered that certain acts of his bring on this anger, he cannot subsequently think of committing one of these acts without thinking of the resulting anger, and feeling more or less of the resulting dread. He has no thought of the utility or inutility of the act itself: the deterrent is the mainly vague, but partially definite, fear of evil that may follow. So understood, the deterring emotion is one which has grown out of experiences of utility, using that word in its ethical sense; and if we ask why this dreaded anger is called forth from others, we shall habitually find that it is because the forbidden act entails pain somewhere—is negatived by utility. On passing from domestic injunctions to injunctions current in the tribe, we see no less clearly how these emotions produced by approbation and reprobation come to be connected in experience with actions which are beneficial to the tribe, and actions which are detrimental to the tribe; and how there consequently grow up incentives to the one class of actions and prejudices against the other class. From early boyhood the young savage hears recounted the daring deeds of his chief—hears them in words of praise, and sees all faces glowing with admiration. From time to time also he listens while some one's cowardice is described in tones of scorn, and with contemptuous metaphors, and sees him meet with derision and insult whenever he appears. That is to say, one of the things that come to be associated in his mind with smiling faces, which are symbolical of pleasures in general, is courage; and one of the things that come to be associated in his mind with frowns and other marks of enmity, which form his symbol of unhappiness, is cowardice. These feelings are not formed in him because he has reasoned his way to the truth that courage is useful to the tribe, and, by implication, to himself, or to the truth that cowardice is a cause of evil. In adult life he may perhaps see this; but he certainly does not see it at the time when bravery is thus joined in his consciousness with all that is good, and cowardice with all that is bad. Similarly there are produced in him feelings of inclination or repugnance towards other lines of conduct that have become established or interdicted, because they are beneficial or injurious to the tribe; though neither the young nor the adults know why they have become established or interdicted. Instance the praiseworthiness of wife-stealing, and the viciousness of marrying within the tribe.

We may now ascend a stage to an order of incentives and restraints derived from these. The primitive belief is that every dead man becomes a demon, who is often somewhere at hand, may at any moment return, may give aid or do mischief, and has to be continually propitiated. Hence among other agents whose approbation or reprobation are contemplated by the savage as consequences of his conduct, are the spirits of his ancestors. When a child he is told of their deeds, now in triumphant tones, now in whispers of horror; and the instilled belief that they may inflict some vaguely-imagined but fearful evil, or give some great help, becomes a powerful incentive or deterrent. Especially does this happen when the story is of a chief, distinguished for his strength, his ferocity, his persistence in that revenge on enemies which the experiences of the savage make him regard as beneficial and virtuous. The consciousness that such a chief, dreaded by neighbouring tribes, and dreaded, too, by members of his own tribe, may reappear and punish those who have disregarded his injunctions, becomes a powerful motive. But it is clear, in the first place, that the imagined anger and the imagined satisfaction of this deified chief, are simply transfigured forms of the anger and satisfaction displayed by those around; and that the feelings accompanying such imaginations have the same original root in the experiences which have associated an average of painful results with the manifestation of another's anger, and an average of pleasurable results with the manifestation of another's satisfaction. And it is clear, in the second place, that the actions thus forbidden and encouraged must be mostly actions that are respectively detrimental and beneficial to the tribe; since the successful chief is usually a better judge than the rest, and has the preservation of the tribe at heart. Hence experiences of utility, consciously or unconsciously organized, underlie his injunctions; and the sentiments which prompt obedience are, though very indirectly and without the knowledge of those who feel them, referable to experiences of utility.

This transfigured form of restraint, differing at first but little from the original form, admits of immense development. Accumulating traditions, growing in grandeur as they are repeated from generation to generation, make more and more superhuman the early-recorded hero of the race. His powers of inflicting punishment and giving happiness become ever greater, more multitudinous, and more varied; so that the dread of divine displeasure, and the desire to obtain divine approbation, acquire a certain largeness and generality. Still the conceptions remain anthropomorphic. The revengeful deity continues to be thought of in terms of human emotions, and continues to be represented as displaying these emotions in human ways. Moreover, the sentiments of right and duty, so far as they have become developed, refer mainly to divine commands and interdicts; and have little reference to the natures of the acts commanded or interdicted. In the intended offering-up of Isaac, in the sacrifice of Jephthah's daughter, and in the hewing to pieces of Agag, as much as in the countless atrocities committed from religious motives by various early historic races, as by some existing savage races, we see that the morality and immorality of actions, as we understand them, are at first little recognized; and that the feelings, chiefly of dread, which serve in place of them, are feelings felt towards the unseen beings supposed to issue the commands and interdicts.

Here it will be said that, as just admitted, these are not the moral sentiments properly so called. They are simply sentiments that precede and make possible those highest sentiments which do not refer either to personal benefits or evils to be expected from men, or to more remote rewards and punishments. Several comments are, however, called forth by this criticism. One is, that if we glance back at past beliefs and their correlative feelings, as shown in Dante's poem, in the mystery-plays of the middle ages, in St. Bartholomew massacres, in burnings for heresy, we get proof that in comparatively modern times right and wrong meant little else than subordination or insubordination—to a divine ruler primarily, and under him to a human ruler. Another is, that down to our own day this conception largely prevails, and is even embodied in elaborate ethical works—instance the *Essays on the Principles of Morality*, by Jonathan Dymond, which recognizes no ground of moral obligation save the will of God as expressed in the current creed. And yet a further is, that while in sermons the torments of the damned and the joys of the blessed are set forth as the dominant deterrents and incentives, and while we have prepared for us printed instructions "how to make the best of both worlds," it cannot be denied that the feelings which impel and restrain men are still largely composed of elements like those operative on the savage: the dread, partly vague, partly specific, associated with the idea of reprobation, human and divine, and the sense of satisfaction, partly vague, partly specific, associated with the idea of approbation, human and divine.

But during the growth of that civilization which has been made possible by these ego-altruistic sentiments, there have been slowly evolving the altruistic sentiments. Development of these has gone on only as fast as society has advanced to a state in which the activities are mainly peaceful. The root of all the altruistic sentiments is sympathy; and sympathy could become dominant only when the mode of life, instead of being one that habitually inflicted direct pain, became one which conferred direct and indirect benefits: the pains inflicted being mainly incidental and indirect. Adam Smith made a large step towards this truth when he recognized sympathy as giving rise to these superior controlling emotions. His *Theory of Moral Sentiments*, however, requires to be supplemented in two ways. The natural process by which sympathy becomes developed into a more and more important element of human nature has to be explained; and there has also to be explained the process by which sympathy produces the highest and most complex of the altruistic sentiments—that of justice. Respecting the first process, I can here do no more than say that sympathy may be proved, both inductively and deductively, to be the concomitant of gregariousness: the two having all along increased by reciprocal aid. Multiplication has ever tended to force into an association, more or less close, all creatures having kinds of food and supplies of food that permit association; and established psychological laws warrant the inference that some sympathy will inevitably result from habitual manifestations of feelings in presence of one another, and that the gregariousness being augmented by the increase of sympathy, further facilitates the development of sympathy. But there are negative and positive checks upon this development—negative, because sympathy cannot advance faster than intelligence advances, since it presupposes the power of interpreting the natural language of the various feelings, and of mentally representing those feelings; positive, because the immediate needs of self-preservation are often at variance with its promptings, as, for example, during the predatory stages of human progress. For explanations of the second process, I must refer to the *Principles of Psychology* (§ 202, first edition, and § 215, second edition) and to *Social Statics*, part ii. chapter v.* Asking that in default of space these explanations may be taken for granted, let me here point out in what sense even sympathy, and the sentiments that result from it, are due to experiences of utility. If we suppose all thought of rewards or punishments, immediate or remote, to be left out of consideration, it is clear that any one who hesitates to inflict a pain because of the vivid representation of that pain which rises in his consciousness, is restrained, not by any sense of obligation or by any formulated doctrine of utility, but by the painful association established in him. And it is clear that if, after repeated experiences of the moral discomfort he has felt from witnessing the unhappiness indirectly caused by some of his acts, he is led to check himself when again tempted to those acts, the restraint is of like nature. Conversely with the pleasure-giving acts: repetitions of kind deeds, and experiences of the sympathetic gratifications that follow, tend continually to make stronger the association between such deeds and feelings of happiness.

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* I may add that in *Social Statics*, chap. xxx., I have indicated, in a general way, the causes of the development of sympathy and the restraints upon its development—confining the discussion, however, to the case of the human race, my subject limiting me to that. The accompanying teleology I now disclaim.

Eventually these experiences may be consciously generalized, and there may result a deliberate pursuit of sympathetic gratifications. There may also come to be distinctly recognized the truths that the remoter results, kind and unkind conduct, are respectively beneficial and detrimental—that due regard for others is conducive to ultimate personal welfare, and disregard of others to ultimate personal disaster; and then there may become current such summations of experience as "honesty is the best policy." But so far from regarding these intellectual recognitions of utility as preceding and causing the moral sentiment, I regard the moral sentiment as preceding such recognitions of utility, and making them possible. The pleasures and pains directly resulting in experience from sympathetic and unsympathetic actions, had first to be slowly associated with such actions, and the resulting incentives and deterrents frequently obeyed, before there could arise the perceptions that sympathetic and unsympathetic actions are remotely beneficial or detrimental to the actor; and they had to be obeyed still longer and more generally before there could arise the perceptions that they are socially beneficial or detrimental. When, however, the remote effects, personal and social, have gained general recognition, are expressed in current maxims, and lead to injunctions having the religious sanction, the sentiments that prompt sympathetic actions and check unsympathetic ones are immensely strengthened by their alliances. Approbation and reprobation, divine and human, come to be associated in thought with the sympathetic and unsympathetic actions respectively. The commands of the creed, the legal penalties, and the code of social conduct, unitedly enforce them; and every child as it grows up, daily has impressed on it by the words and faces and voices of those around the authority of these highest principles of conduct. And now we may see why there arises a belief in the special sacredness of these highest principles, and a sense of the supreme authority of the altruistic sentiments answering to them. Many of the actions which, in early social states, received the religious sanction and gained public approbation, had the drawback that such sympathies as existed were outraged, and there was hence an imperfect satisfaction. Whereas these altruistic actions, while similarly having the religious sanction and gaining public approbation, bring a sympathetic consciousness of pleasure given or of pain prevented; and, beyond this, bring a sympathetic consciousness of human welfare at large, as being furthered by making altruistic actions habitual. Both this special and this general sympathetic consciousness become stronger and wider in proportion as the power of mental representation increases, and the imagination of consequences, immediate and remote, grows more vivid and comprehensive. Until at length these altruistic sentiments begin to call in question the authority of those ego-altruistic sentiments which once ruled unchallenged. They prompt resistance to laws that do not fulfil the conception of justice, encourage men to brave the frowns of their fellows by pursuing a course at variance with customs that are perceived to be socially injurious, and even cause dissent from the current religion; either to the extent of disbelief in those alleged divine attributes and acts not approved by this suprême moral arbiter, or to the extent of entire rejection of a creed which ascribes such attributes and acts.

Much that is required to make this hypothesis complete must stand over until, at the close of the second volume of the *Principles of Psychology*, I have space for a full exposition. What I have said will make it sufficiently clear that two fundamental errors have been made in the interpretation put upon it. Both Utility and Experience have been construed in senses much too narrow. Utility, convenient a word as it is from its comprehensiveness, has very inconvenient and misleading implications. It vividly suggests uses, and means, and proximate ends, but very faintly suggests the pleasures, positive or negative, which are the ultimate ends, and which, in the ethical meaning of the word, are alone considered; and, further, it implies conscious recognition of means and ends — implies the deliberate taking of some course to gain a perceived benefit. Experience, too, in its ordinary acceptation, connotes definite perceptions of causes and consequences, as standing in observed relations, and is not taken to include the connexions formed in consciousness between states that recur together, when the relation between them, causal or other, is not perceived. It is in their widest senses, however, that I habitually use these words, as will be manifest to every one who reads the *Principles of Psychology;* and it is in their widest senses that I have used them in the letter to Mr. Mill. I think I have shown above that, when they are so understood, the hypothesis briefly set forth in that letter is by no means so indefensible as is supposed. At any rate, I have shown—what seemed for the present needful to show—that Mr. Hutton's versions of my views must not be accepted as correct.

# THE COMPARATIVE PSYCHOLOGY OF MAN.

[*Originally read before the Anthropological Institute, and afterwards published in* Mind, *for January*, 1876.]

While discussing with two members of the Anthropological Institute the work to be undertaken by its psychological section, I made certain suggestions which they requested me to put in writing. When reminded, some months after, of the promise I had made to do this, I failed to recall the particular suggestions referred to; but in the endeavour to remember them, I was led to glance over the whole subject of comparative human psychology. Hence resulted the following paper.

That making a general survey is useful as a preliminary to deliberate study, either of a whole or of any part, scarcely needs showing. Vagueness of thought accompanies the wandering about in a region without known bounds or landmarks. Attention devoted to some portion of a subject in ignorance of its connexion with the rest, leads to untrue conceptions. The whole cannot be rightly conceived without some knowledge of the parts; and no part can be rightly conceived out of relation to the whole.

To map out the Comparative Psychology of Man must also conduce to the more methodic carrying on of inquiries. In this, as in other things, division of labour will facilitate progress; and that there may be division of labour, the work itself must be systematically divided.

We may conveniently separate the entire subject into three main divisions, and may arrange them in the order of increasing speciality.

The first division will treat of the degrees of mental evolution of different human types, generally considered: taking account of both the mass of mental manifestation and the complexity of mental manifestation. This division will include the relations of these characters to physical characters—the bodily mass and structure, and the cerebral mass and structure. It will also include inquiries concerning the time taken in completing mental evolution, and the time during which adult mental power lasts; as well as certain most general traits of mental action, such as the greater or less persistence of emotions and of intellectual processes. The connexion between the general mental type and the general social type should also be here dealt with.

In the second division may be conveniently placed apart, inquiries concerning the relative mental natures of the sexes in each race. Under it will come such questions as these:—What differences of mental mass and mental complexity, if any, existing between males and females, are common to all races? Do such differences vary in degree, or in kind, or in both? Are there reasons for thinking that they are liable to change by increase or decrease? What relations do they bear in each case to the habits of life, the domestic arrangements, and the social arrangements? This division should also include in its scope the sentiments of the sexes towards one another, considered as varying quantitatively and qualitatively; as well as their respective sentiments towards offspring, similarly varying.

For the third division of inquiries may be reserved the more special mental traits distinguishing different types of men. One class of such specialities results from differences of proportion among faculties possessed in common; and another class results from the presence in some races of faculties that are almost or quite absent from others. Each difference in each of these groups, when established by comparison, has to be studied in connexion with the stage of mental evolution reached, and has to be studied in connexion with the habits of life and the social development, regarding it as related to these both as cause and as consequence.

Such being the outlines of these several divisions, let us now consider in detail the subdivisions contained within each.

I.—Under the head of general mental evolution we may begin with the trait of—

1. *Mental mass.*—Daily experiences show us that human beings differ in volume of mental manifestation. Some there are whose intelligence, high though it may be, produces little impression on those around; while there are some who, when uttering even commonplaces, do it so as to affect listeners in a disproportionate degree. Comparison of two such, makes it manifest that, generally, the difference is due to the natural language of the emotions. Behind the intellectual quickness of the one there is not felt any power of character; while the other betrays a momentum capable of bearing down opposition—a potentiality of emotion that has something formidable about it. Obviously the varieties of mankind differ much in respect of this trait. Apart from kind of feeling, they are unlike in amount of feeling. The dominant races overrun the inferior races mainly in virtue of the greater quantity of energy in which this greater mental mass shows itself. Hence a series of inquiries, of which these are some:—(*a*) What is the relation between mental mass and bodily mass? Manifestly, the small races are deficient in it. But it also

appears that races much upon a par in size—as, for instance, an Englishman and a Damara, differ considerably in mental mass. (*b*) What is its relation to mass of brain? and, bearing in mind the general law that in the same species, size of brain increases with size of body (though not in the same proportion), how far can we connect the extra mental mass of the higher races, with an extra mass of brain beyond that which is proper to their greater bodily mass? (*c*) What relation, if any, is there between mental mass and the physiological state expressed in vigour of circulation and richness of blood, as severally determined by mode of life and general nutrition? (*d*) What are the relations of this trait to the social state, as nomadic or settled, predatory or industrial?

2. *Mental complexity.*—How races differ in respect of the more or less involved structures of their minds, will best be understood on recalling the unlikeness between the juvenile mind and the adult mind among ourselves. In the child we see absorption in special facts. Generalities even of a low order are scarcely recognized, and there is no recognition of high generalities. We see interest in individuals, in personal adventures, in domestic affairs, but no interest in political or social matters. We see vanity about clothes and small achievements, but little sense of justice: witness the forcible appropriation of one another's toys. While there have come into play many of the simpler mental powers, there has not yet been reached that complication of mind which results from the addition of powers evolved out of these simpler ones. Kindred differences of complexity exist between the minds of lower and higher races; and comparisons should be made to ascertain their kinds and amounts. Here, too, there may be a subdivision of the inquiries. (*a*) What is the relation between mental complexity and mental mass? Do not the two habitually vary together? (*b*) What is the relation to the

social state, as more or less complex? that is to say—Do not mental complexity and social complexity act and react on each other?

3. *Rate of mental development.*—In conformity with the biological law that the higher the organisms the longer they take to evolve, members of the inferior human races may be expected to complete their mental evolution sooner than members of the superior races; and we have evidence that they do this. Travellers from many regions comment, now on the great precocity of children among savage and semi-civilized peoples, and now on the early arrest of their mental progress. Though we scarcely need more proofs that this general contrast exists, there remains to be asked the question, whether it is consistently maintained throughout all groups of races, from the lowest to the highest—whether, say, the Australian differs in this respect from the Hindu, as much as the Hindu does from the European. Of secondary inquiries coming under this sub-head may be named several. (*a*) Is this more rapid evolution and earlier arrest always unequally shown by the two sexes; or, in other words, are there in lower types proportional differences in rate and degree of development, such as higher types show us? (*b*) Is there in many cases, as there appears to be in some cases, a traceable relation between the period of arrest and the period of puberty? (*c*) Is mental decay early in proportion as mental evolution is rapid? (*d*) Can we in other respects assert that where the type is low, the entire cycle of mental changes between birth and death—ascending, uniform, descending—comes within a shorter interval?

4. *Relative plasticity.*—Is there any relation between the degree of mental modifiability which remains in adult life, and the character of the mental evolution in respect of mass, complexity, and rapidity? The animal kingdom at large yields reasons for associating an inferior and more rapidly-completed mental structure, with a relatively automatic

nature. Lowly organized creatures, guided almost entirely by reflex actions, are in but small degrees changeable by individual experiences. As the nervous structure complicates, its actions become less rigorously confined within pre-established limits; and as we approach the highest creatures, individual experiences take larger and larger shares in moulding the conduct: there is an increasing ability to take in new impressions and to profit by the acquisitions. Inferior and superior human races are contrasted in this respect. Many travellers comment on the unchangeable habits of savages. The semi-civilized nations of the East, past and present, were, or are, characterized by a greater rigidity of custom than characterizes the more civilized nations of the West. The histories of the most civilized nations show us that in their earlier times, the modifiability of ideas and habits was less than it is at present. And if we contrast classes or individuals around us, we see that the most developed in mind are the most plastic. To inquiries respecting this trait of comparative plasticity, in its relations to precocity and early completion of mental development, may fitly be added inquiries respecting its relations to the social state, which it helps to determine, and which reacts upon it.

5. *Variability.*—To say of a mind that its actions are extremely inconstant, and at the same time to say that it is of relatively unchangeable nature, apparently implies a contradiction. When, however, the inconstancy is understood as referring to the manifestations which follow one another from minute to minute, and the unchangeableness to the average manifestations, extending over long periods, the apparent contradiction disappears; and it becomes comprehensible that the two traits may, and ordinarily do, coexist. An infant, quickly wearied with each kind of perception, wanting ever a new object which it soon abandons for something else, and alternating a score times a day between smiles and tears, shows us a very small persistence

in each kind of mental action: all its states, intellectual and emotional, are transient. Yet at the same time its mind cannot be easily changed in character. True, it changes spontaneously in due course; but it long remains incapable of receiving ideas or emotions beyond those of simple orders. The child exhibits less rapid variations, intellectual and emotional, while its educability is greater. Inferior human races show us this combination: great rigidity of general character with great irregularity in its passing manifestations. Speaking broadly, while they resist permanent modification, they lack intellectual persistence, and they lack emotional persistence. Of various low types we read that they cannot keep the attention fixed beyond a few minutes on anything requiring thought, even of a simple kind. Similarly with their feelings: these are less enduring than those of civilized men. There are, however, qualifications to be made in this statement; and comparisons are needed to ascertain how far these qualifications go. The savage shows great persistence in the action of the lower intellectual faculties. He is untiring in minute observation. He is untiring, also, in that kind of perceptive activity which accompanies the making of his weapons and ornaments: often persevering for immense periods in carving stones, &c. Emotionally, too, he shows persistence not only in the motives prompting these small industries, but also in certain of his passions—especially in that of revenge. Hence, in studying the degrees of mental variability shown us in the daily lives of the different races, we must ask how far variability characterizes the whole mind, and how far it holds only of parts of the mind.

6. *Impulsiveness.*—This trait is closely allied with the last: unenduring emotions are emotions which sway the conduct now this way and now that, without any consistency. The trait of impulsiveness may, however, be fitly dealt with separately, because it has other implications than mere lack of persistence. Comparisons of the lower human

races with the higher, appear generally to show that, along with brevity of the passions, there goes violence. The sudden gusts of feeling which men of inferior types display, are excessive in degree as they are short in duration; and there is probably a connexion between these two traits: intensity sooner producing exhaustion. Observing that the passions of childhood illustrate this connexion, let us turn to certain interesting questions concerning the decrease of impulsiveness which accompanies advance in evolution. The nervous processes of an impulsive being, are less remote from reflex actions than are those of an unimpulsive being. In reflex actions we see a simple stimulus passing suddenly into movement: little or no control being exercised by other parts of the nervous system. As we ascend to higher actions, guided by more and more complicated combinations of stimuli, there is not the same instantaneous discharge in simple motions; but there is a comparatively deliberate and more variable adjustment of compound motions, duly restrained and proportioned. It is thus with the passions and sentiments in the less developed natures and in the more developed natures. Where there is but little emotional complexity, an emotion, when excited by some occurrence, explodes in action before the other emotions have been called into play; and each of these, from time to time, does the like. But the more complex emotional structure is one in which these simpler emotions are so co-ordinated that they do not act independently. Before excitement of any one has had time to cause action, some excitement has been communicated to others—often antagonistic ones; and the conduct becomes modified in adjustment to the combined dictates. Hence results a decreased impulsiveness, and also a greater persistence. The conduct pursued, being prompted by several emotions co-operating in degrees which do not exhaust them, acquires a greater continuity; and while spasmodic force becomes less conspicuous, there is an increase in the total energy. Examining the facts from this point of view, there are sundry questions of interest to be put respecting the different races of men. (*a*) To what other traits than degree of mental evolution is impulsiveness related? Apart from difference in elevation of type, the New-World races seem to be less impulsive than the Old-World races. Is this due to constitutional apathy? Can there be traced (other things equal) a relation between physical vivacity and mental impulsiveness? (*b*) What connexion is there between this trait and the social state? Clearly a very explosive nature—such as that of the Bushman—is unfit for social union; and, commonly, social union, when by any means established, checks impulsiveness. (*c*) What respective shares in checking impulsiveness are taken by the feelings which the social state fosters—such as the fear of surrounding individuals, the instinct of sociality, the desire to accumulate property, the sympathetic feelings, the sentiment of justice? These, which require a social environment for their development, all of them involve imaginations of consequences more or less distant; and thus imply checks upon the promptings of the simpler passions. Hence arise the questions—In what order, in what degrees, and in what combinations, do they come into play?

7. One further general inquiry of a different kind may be added. What effect is produced on mental nature by mixture of races? There is reason for believing that throughout the animal kingdom, the union of varieties which have become widely divergent is physically injurious; while the union of slightly divergent varieties is physically beneficial. Does the like hold with the mental nature? Some facts seem to show that mixture of human races extremely unlike, produces a worthless type of mind—a mind fitted neither for the kind of life led by the higher of the two races, not for that led by the lower—a mind out of adjustment to all conditions of life. Contrariwise, we find that peoples of the same stock, slightly differenti-

ated by lives carried on in unlike circumstances for many generations, produce by mixture a mental type having certain superiorities. In his work on *The Huguenots,* Mr. Smiles points out how large a number of distinguished men among us have descended from Flemish and French refugees; and M. Alphonse de Candolle, in his *Histoire des Sciences et des Savants depuis deux Siècles,* shows that the descendants of French refugees in Switzerland have produced an unusually great proportion of scientific men. Though, in part, this result may be ascribed to the original natures of such refugees, who must have had that independence which is a chief factor in originality, yet it is probably in part due to mixtures of races. For thinking this, we have evidence which is not open to two interpretations. Prof. Morley draws attention to the fact that, during seven hundred years of our early history "the best genius of England sprang up on the line of country in which Celts and Anglo-Saxons came together." In like manner Mr. Galton, in his *English Men of Science,* shows that in recent days these have mostly come from an inland region, running generally from north to south, which we may reasonably presume contains more mixed blood than do the regions east and west of it. Such a result seems probable *a priori.* Two natures respectively adapted to slightly unlike sets of social conditions, may be expected by their union to produce a nature somewhat more plastic than either—a nature more impressible by the new circumstances of advancing social life, and therefore more likely to originate new ideas and display modified sentiments. The Comparative Psychology of Man may, then, fitly include the mental effects of mixture; and among derivative inquiries we may ask—How far the conquest of race by race has been instrumental in advancing civilization by aiding mixture, as well as in other ways.

II.—The second of the three leading divisions named at the outset is less extensive. Still, concerning the relative mental natures of the sexes in each raçe, questions of much interest and importance may be raised.

1. *Degree of difference between the sexes.*—It is an established fact that, physically considered, the contrast between males and females is not equally great in all types of mankind. The bearded races, for instance, show us a greater unlikeness between the two than do the beardless races. Among South American tribes, men and women have a greater general resemblance in form, &c., than is usual elsewhere. The question, then, suggests itself—Do the mental natures of the sexes differ in a constant or in a variable degree? The difference is unlikely to be a constant one; and, looking for variation, we may ask what is its amount, and under what conditions does it occur?

2. *Difference in mass and in complexity.*—The comparisons between the sexes, of course, admit of subdivisions parallel to those made in the comparisons between races. Relative mental mass and relative mental complexity have chiefly to be observed. Assuming that the great inequality in the cost of reproduction to the two sexes, is the cause of unlikeness in mental mass, as in physical mass, this difference may be studied in connexion with reproductive differences presented by the various races, in respect of the ages at which reproduction commences, and the periods over which it lasts. An allied inquiry may be joined with this; namely, how far the mental developments of the two sexes are affected by their relative habits in respect to food and physical exertion? In many of the lower races, the women, treated with great brutality, are, physically, much inferior to the men: excess of labour and defect of nutrition being apparently the combined causes. Is any arrest of mental development simultaneously caused?

3. *Variation of the differences.*—If the unlikeness,

physical and mental, of the sexes is not constant, then, supposing all races have diverged from one original stock, it follows that there must have been transmission of accumulated differences to those of the same sex in posterity. If, for instance, the prehistoric type of man was beardless, then the production of a bearded variety implies that within that variety the males continued to transmit an increasing amount of beard to descendants of the same sex. This limitation of heredity by sex, shown us in multitudinous ways throughout the animal kingdom, probably applies to the cerebral structures as much as to other structures. Hence the question—Do not the mental natures of the sexes in alien types of Man diverge in unlike ways and degrees?

4. *Causes of the differences.*—Are any relations to be traced between these variable differences and the variable parts the sexes play in the business of life? Assuming the cumulative effects of habit on function and structure, as well as the limitation of heredity by sex, it is to be expected that if, in any society, the activities of one sex, generation after generation, differ from those of the other, there will arise sexual adaptations of mind. Some instances in illustration may be named. Among the Africans of Loango and other districts, as also among some of the Indian Hill-tribes, the men and women are strongly contrasted as respectively inert and energetic: the industry of the women having apparently become so natural to them that no coercion is needed. Of course, such facts suggest an extensive series of questions. Limitation of heredity by sex may account both for those sexual differences of mind which distinguish men and women in all races, and for those which distinguish them in each race, or each society. An interesting subordinate inquiry may be, how far such mental differences are inverted in cases where there is inversion of social and domestic

relations; as among those Khasi Hill-tribes, whose women have so far the upper hand that they turn off their husbands in a summary way if they displease them.

5. *Mental modifiability in the two sexes.*—Along with comparisons of races in respect of mental plasticity may go parallel comparisons of the sexes in each race. Is it true always, as it appears to be generally true, that women are less modifiable than men? The relative conservatism of women—their greater adhesion to established ideas and practices—is manifest in many civilized and semi-civilized societies. Is it so among the uncivilized? A curious instance of stronger attachment to custom in women than in men is given by Dalton, as occurring among the Juangs, one of the lowest wild tribes of Bengal. Until recently the only dress of both sexes was something less than that which the Hebrew legend gives to Adam and Eve. Years ago the men were led to adopt a cloth bandage round the loins, in place of the bunch of leaves; but the women adhered to the aboriginal habit: a conservatism shown where it might have been least expected.

6. *The sexual sentiment.*—Results of value may be looked for from comparisons of races made to determine the amounts and characters of the higher feelings to which the relation of the sexes gives rise. The lowest varieties of mankind have but small endowments of these feelings. Among varieties of higher types, such as the Malayo-Polynesians, these feelings seem considerably developed: the Dyaks, for instance, sometimes display them in great strength. Speaking generally, they appear to become stronger with the advance of civilization. Several subordinate inquiries may be named. (*a*) How far is development of the sexual sentiment dependent upon intellectual advance—upon growth of imaginative power? (*b*) How far is it related to emotional advance; and especially to evolution of those emotions which originate from sympathy? What are its relations to polyandry and polygyny? (*c*)

Does it not tend towards, and is it not fostered by, monogamy? (*d*) What connexion has it with maintenance of the family bond, and the consequent better rearing of children?

III.—Under the third head, to which we may now pass come the more special traits of the different races.

1. *Imitativeness.*—One of the characteristics in which the lower types of men show us a smaller departure from reflex action than do the higher types, is their strong tendency to mimic the motions and sounds made by others—an almost involuntary habit which travellers find it difficult to check. This meaningless repetition, which seems to imply that the idea of an observed action cannot be framed in the mind of the observer without tending forthwith to discharge itself in the action conceived (and every ideal action is a nascent form of the consciousness accompanying performance of such action), evidently diverges but little from the automatic; and decrease of it is to be expected along with increase of self-regulating power. This trait of automatic mimicry is evidently allied with that less automatic mimicry which shows itself in greater persistence of customs. For customs adopted by each generation from the last without thought or inquiry, imply a tendency to imitate which overmasters critical and sceptical tendencies: so maintaining habits for which no reasons can be given. The decrease of this irrational mimicry, strongest in the lowest savage and feeblest in the highest of the civilized, should be studied along with the successively higher stages of social life, as being at once an aid and a hindrance to civilization: an aid in so far as it gives that fixity to the social organization without which a society cannot survive; a hindrance in so far as it offers resistance to changes of social organization that have become desirable.

2. *Incuriosity.*—Projecting our own natures into the circumstances of the savage, we imagine ourselves as

marvelling greatly on first seeing the products and appliances of civilized life. But we err in supposing that the savage has feelings such as we should have in his place. Want of rational curiosity respecting these incomprehensible novelties, is a trait remarked of the lowest races wherever found; and the partially-civilized races are distinguished from them as exhibiting rational curiosity. The relation of this trait to the intellectual nature, to the emotional nature, and to the social state, should be studied.

3. *Quality of thought.*—Under this vague head may be placed many sets of inquiries, each of them extensive—(*a*) The degree of generality of the ideas; (*b*) the degree of abstractness of the ideas; (*c*) the degree of definiteness of the ideas; (*d*) the degree of coherence of the ideas; (*e*) the extent to which there have been developed such notions as those of *class*, of *cause*, of *uniformity*, of *law*, of *truth*. Many conceptions which have become so familiar to us that we assume them to be the common property of all minds, are no more possessed by the lowest savages than they are by our own children; and comparisons of types should be so made as to elucidate the processes by which such conceptions are reached. The development under each head has to be observed—(*a*) independently in its successive stages; (*b*) in connexion with the co-operative intellectual conceptions; (*c*) in connexion with the progress of language, of the arts, and of social organization. Already linguistic phenomena have been used in aid of such inquiries; and more systematic use of them should be made. Not only the number of general words, and the number of abstract words, in a people's vocabulary should be taken as evidence, but also their *degrees* of generality and abstractness; for there are generalities of the first, second, third, &c., orders, and abstractions similarly ascending. *Blue* is an abstraction referring to one class of impressions derived from visible objects; *colour* is a higher abstraction referring to many such classes of visual impressions; *property* is a still

higher abstraction referring to classes of impressions received not through the eyes alone, but through other sense-organs. If generalities and abstractions were arranged in the order of their extensiveness and in the order of their grades, tests would be obtained which, applied to the vocabularies of the uncivilized, would yield definite evidence of the intellectual stages reached.

4. *Peculiar aptitudes.*—To such specialities of intelligence as mark different degrees of evolution, have to be added minor ones related to modes of life: the kinds and degrees of faculty which have become organized in adaptation to daily habits—skill in the use of weapons, powers of tracking, quick discrimination of individual objects. And under this head may fitly come inquiries concerning some race-peculiarities of the æsthetic class, not at present explicable. While the remains from the Dordogne caves show us that their inhabitants, low as we must suppose them to have been, could represent animals, both by drawing and carving, with some degree of fidelity; there are existing races, probably higher in other respects, who seem scarcely capable of recognizing pictorial representations. Similarly with the musical faculty. Almost or quite wanting in some inferior races, we find it in other races not of high grade, developed to an unexpected degree: instance the Negroes, some of whom are so innately musical, that, as I have been told by a missionary among them, the children in native schools when taught European psalm-tunes, spontaneously sing seconds to them. Whether any causes can be discovered for race peculiarities of this kind, is a question of interest.

5. *Specialities of emotional nature.*—These are worthy of careful study, as being intimately related to social phenomena—to the possibility of social progress, and to the nature of the social structure. Among others to be noted there are—(*a*) Gregariousness or sociality—a trait in the strength of which races differ widely: some, as the

Mantras, being almost indifferent to social intercourse; some being unable to dispense with it. Obviously the degree of this desire for the presence of fellow-men, affects greatly the formation of social groups, and consequently influences social progress. (*b*) Intolerance of restraint. Men of some inferior types, as the Mapuché, are ungovernable; while those of other types, no higher in grade, not only submit to restraint, but admire the persons exercising it. These contrasted natures have to be observed in connexion with social evolution; to the early stages of which they are respectively antagonistic and favourable. (*c*) The desire for praise is a trait which, common to all races, high and low, varies considerably in degree. There are quite inferior races, as some of those in the Pacific States, whose members sacrifice without stint to gain the applause which lavish generosity brings; while, elsewhere, applause is sought with less eagerness. Notice should be taken of the connexion between this love of approbation and the social restraints; since it plays an important part in the maintenance of them. (*d*) The acquisitive propensity. This, too, is a character the degrees of which, and the relations of which to the social state, have to be especially noted. The desire for property grows along with the possibility of gratifying it; and this, extremely small among the lowest men, increases as social development goes on. With the advance from tribal property to family property and individual property, the notion of private right of possession gains definiteness, and the love of acquisition strengthens. Each step towards an orderly social state makes larger accumulations possible, and the pleasures achievable by them more sure; while the resulting encouragement to accumulate, leads to increase of capital and to further progress. This action and re-action of the sentiment and the social state, should be in every case observed.

6. *The altruistic sentiments.*—Coming last, these are also

highest. The evolution of them in the course of civilization, shows us clearly the reciprocal influences of the social unit and the social organism. On the one hand, there can be no sympathy, nor any of the sentiments which sympathy generates, unless there are fellow-beings around. On the other hand, maintenance of union with fellow-beings depends in part on the presence of sympathy, and the resulting restraints on conduct. Gregariousness or sociality favours the growth of sympathy; increased sympathy conduces to closer sociality and a more stable social state; and so, continuously, each increment of the one makes possible a further increment of the other. Comparisons of the altruistic sentiments resulting from sympathy, as exhibited in different types of men and different social states, may be conveniently arranged under three heads — (*a*) Pity, which should be observed as displayed towards offspring, towards the sick and aged, and towards enemies. (*b*) Generosity (duly discriminated from the love of display) as shown in giving; as shown in the relinquishment of pleasures for the sake of others; as shown by active efforts on others' behalf. The manifestations of this sentiment, too, are to be noted in respect of their range —whether they are limited to relatives; whether they extend only to those of the same society; whether they extend to those of other societies; and they are also to be noted in connexion with the degree of providence— whether they result from sudden impulses obeyed without counting the cost, or go along with clear foresight of the future sacrifices entailed. (*c*) Justice. This most abstract of the altruistic sentiments is to be considered under aspects like those just named, as well as under many other aspects—how far it is shown in regard to the lives of others; how far in regard to their freedom; how far in regard to their property; how far in regard to their various minor claims. And comparisons concerning this highest sentiment should, beyond all others, be carried on along with comparisons of the accompanying social states, which it largely determines—the forms and actions of governments; the characters of laws; the relations of classes.

Such, stated as briefly as consists with clearness, are the leading divisions and subdivisions under which the Comparative Psychology of Man may be arranged. In going rapidly over so wide a field, I have doubtless overlooked much that should be included. Doubtless, too, various of the inquiries named will branch out into subordinate inquiries well worth pursuing. Even as it is, however, the programme is extensive enough to occupy numerous investigators, who may with advantage take separate divisions.

Though, after occupying themselves with primitive arts and products, anthropologists have devoted their attention mainly to the physical characters of the human races; it must, I think, be admitted that the study of these yields in importance to the study of their psychical characters. The general conclusions to which the first set of inquiries may lead, cannot so much affect our views respecting the highest classes of phenomena as can the general conclusions to which the second set may lead. A true theory of the human mind vitally concerns us; and systematic comparisons of human minds, differing in their kinds and grades, will help us in forming a true theory. Knowledge of the reciprocal relations between the characters of men and the characters of the societies they form, must influence profoundly our ideas of political arrangements. When the inter-dependence of individual natures and social structures is understood, our conceptions of the changes now taking place, and hereafter to take place, will be rectified. A comprehension of mental development as a process of adaptation to social conditions, which are continually remoulding the mind and are again remoulded by it, will conduce to a salutary consciousness of the remoter effects produced by institutions upon character; and will check the grave mischiefs which ignorant legislation now causes. Lastly, a right theory of mental evolution as exhibited by humanity at large, giving a key, as it does, to the evolution of the individual mind, must help to rationalize our perverse methods of education; and so to raise intellectual power and moral nature.

# MR. MARTINEAU ON EVOLUTION.

[*First published in* The Contemporary Review, *for June*, 1872.]

THE article by Mr. Martineau, in the April number of the *Contemporary Review*, on "The Place of Mind in Nature, and Intuition of Man," recalled to me a partially-formed intention to deal with the chief criticisms which have from time to time been made on the general doctrine set forth in *First Principles;* since, though not avowedly directed against propositions asserted or implied in that work, Mr. Martineau's reasoning tells against them by implication. The fulfilment of this intention I should, however, have continued to postpone, had I not learned that the arguments of Mr. Martineau are supposed by many to be conclusive, and that, in the absence of replies, it will be assumed that no replies can be made. It seems desirable, therefore, to notice these arguments at once—especially as the essential ones may, I think, be effectually dealt with in a comparatively small space.

The first definite objection which Mr. Martineau raises is, that the hypothesis of General Evolution is powerless to account even for the simpler orders of facts in the absence of numerous different substances. He argues that were matter all of one kind, no such phenomena as chemical changes would be possible; and that, "in order to start the world on its chemical career, you must enlarge its capital and present it with an outfit of *heterogeneous* constituents. Try, therefore, the effect of such a gift; fling into the pre-existing cauldron the whole list of recognized elementary substances, and give leave to their affinities to work." The intended implication obviously is, that there must exist the separately-created elements before evolution can begin.

Here, however, Mr. Martineau makes an assumption which few, if any, chemists will commit themselves to, and which many will distinctly deny. There are no "recognized elementary substances," if the expression means substances known to be elementary. What chemists, for convenience, call elementary substances, are merely substances which they have thus far failed to decompose; but, bearing in mind past experiences, they do not dare to say that they are absolutely undecomposable. Water was taken to be an element for more than two thousand years, and then was proved to be a compound; and, until Davy brought a galvanic current to bear upon them, the alkalies and the earths were supposed to be elements. So little true is it that "recognized elementary substances" are supposed to be absolutely elementary, that there has been much speculation among chemists respecting the process of compounding and recompounding by which they have been formed out of some ultimate substance—some chemists having supposed the atom of hydrogen to be the unit of composition, but others having contended that the atomic weights of the so-called elements are not thus interpretable. If I remember rightly, Sir John Herschel was one, among others, who, some five-and-twenty years ago, threw out suggestions respecting a system of compounding that might explain these relations of the atomic weights.

What was at that time a suspicion has now become practically a certainty. Spectrum-analysis yields results wholly irreconcilable with the assumption that the conventionally-named simple substances are really simple. Each yields a spectrum having lines varying in number from two to eighty or more, every one of which implies the intercepting of ethereal undulations of a certain order by something oscillating in unison or in harmony with them. Were iron absolutely elementary, it is not conceivable that its atom could intercept ethereal undulations of eighty different orders. Though it does not follow that its molecule contains as many separate atoms as there are lines in its spectrum, it must clearly be a complex molecule. The evidence thus gained points to the conclusion that, out of some primordial units, the so-called elements arise by compounding and recompounding; just as by the compounding and recompounding of so-called elements there arise oxides, and acids, and salts.

And this hypothesis is entirely in harmony with the phenomena of allotropy. Various substances, conventionally distinguished as simple, have several forms under which they present quite different properties. The semi-transparent, colourless, extremely active substance called phosphorus may be so changed as to become opaque, dark red, and inert. Like changes are known to occur in some gaseous, non-metallic elements, as oxygen; and also in metallic elements, as antimony. These total changes of properties, brought about without any changes to be called chemical, are interpretable only as due to molecular re-arrangements; and, by showing that difference of property is producible by difference of arrangement, they support the inference otherwise to be drawn, that the properties of different elements result from differences of arrangement arising by the compounding and recompounding of ultimate homogeneous units.

Thus Mr. Martineau's objection, which at best would imply a turning of our ignorance of the nature of elements into positive knowledge that they are simple, is, in fact, to be met by two sets of evidences, which imply that they are compound.

Mr. Martineau next alleges that a fatal difficulty is put in the way of the General Doctrine of Evolution by the existence of a chasm between the living and the not-living. He says:—"But with all your enlargement of data, turn them as you will, at the end of every passage which they explore, the *door of life* is closed against them still." Here again our ignorance is employed to play the part of knowledge. The fact that we do not know distinctly how an alleged transition has taken place, is transformed into the assumption that no transition has taken place. We have, in a more general shape, the argument which until lately was thought conclusive—the argument that because the genesis of each species of creature had not been explained, therefore each species must have been separately created.

Merely noting this, however, I go on to remark that scientific discovery is day by day narrowing the chasm, or, to vary Mr. Martineau's metaphor, "opening the door." Not many years since, it was held as certain that the chemical compounds distinguished as organic could not be formed artificially. Now, more than a thousand organic compounds have been formed artificially. Chemists have discovered the art of building them up from the simpler to the more complex, and do not doubt that they will eventually produce the most complex. Moreover, the phenomena attending isomeric change give a clue to those movements which are the only indications we have of life in its lowest forms. In various colloidal substances, including the albuminoid, isomeric change is accompanied by contraction or expansion, and consequent motion; and, in such primordial types as the *Protogenes* of Haeckel, which do not differ in appearance from minute portions of albumen, the observed motions are comprehensible as accompanying isomeric changes caused by variations in surrounding physical actions. The probability of this interpretation will be seen on remembering the evidence we have that, in the higher organisms, many functions are essentially effected by isomeric changes from one to another of the multitudinous forms which protein assumes.

Thus the reply to this objection is, first, that there is going on from both sides a narrowing of the chasm supposed to be impassable; and, secondly, that, even were the chasm not in course of being filled up, we should no more be justified in therefore assuming a supernatural commencement of life, than Kepler was justified in assuming that there were guiding-spirits to keep the planets in their orbits, because he could not see how else they were to be kept in their orbits.

The third definite objection made by Mr. Martineau is of kindred nature. The Hypothesis of Evolution is, he thinks, met by the insurmountable difficulty that plant life and animal life are absolutely distinct. "You cannot," he says, "take a single step toward the deduction of sensation and thought: neither at the upper limit do the highest plants (the exogens) transcend themselves and overbalance into animal existence; nor at the lower, grope as you may among the sea-weeds and sponges, can you persuade the sporules of the one to develop into the other."

This is an extremely unfortunate objection to raise. For, though there are no transitions from vegetal to animal life at the places Mr. Martineau names, where, indeed, no biologist would look for them; yet the connexion between the two great kingdoms of living things is so complete that separation is now regarded as impossible. For a long time naturalists endeavored to frame definitions such as would, the one include all plants and exclude all animals, and the other include all animals and exclude all plants. But they have been so repeatedly foiled in the attempt that they have given it up. There is no chemical distinction which holds; there is no structural distinction which holds; there is no functional distinction which holds; there is no distinction as to mode of existence which holds. Large groups of the simpler animals contain chlorophyll, and decompose carbonic acid under the influence of light, as plants do. Large groups of the simpler plants, as you may observe in the diatoms from any stagnant pool, are no less actively locomotive than the minute creatures classed as animals seen along with them. Nay, among these lowest types of living things, it is common for the life to be now predominantly animal and presently to become predominantly vegetal. The very name *zoospores*, given to germs of *algæ*, which for a while swim about actively by means of cilia, and presently settling down grow into plant-forms, is given because of this conspicuous community of nature. So complete is this community of nature that for some time past many naturalists have wished to establish for these lowest types a sub-kingdom, intermediate between the animal and the vegetal: the reason against this course being, however, that the difficulty crops up afresh at any assumed places where this intermediate sub-kingdom may be supposed to join the other two.

Thus the assumption on which Mr. Martineau proceeds is diametrically opposed to the conviction of naturalists in general.

Though I do not perceive that it is specifically stated, there appears to be tacitly implied a fourth difficulty of allied kind—the difficulty that there is no possibility of transition from life of the simplest kind to mind. Mr. Martineau says, indeed, that there can be "with only vital resources, as in the vegetable world, no beginning of mind:" apparently leaving it to be inferred that in the animal world the resources are such as to make the "beginning of mind" comprehensible. If, however, instead of leaving it a latent inference, he had distinctly asserted a chasm between mind and bodily life, for which there is certainly quite as much reason as for asserting a chasm between animal life and vegetal life, the difficulties in his way would have been no less insuperable.

For those lowest forms of irritability in the animal kingdom which, I suppose, Mr. Martineau refers to as the "beginning of mind," are not distinguishable from the irritability which plants display: they in no greater degree imply consciousness. If the sudden folding of a sensitive-plant's leaf when touched, or the spreading out of the stamens in a wild-cistus when gently brushed, is to be considered a vital action of a purely physical kind; then so too must be considered the equally slow contraction of a polype's tentacles. And yet, from this simple motion of an animal of low type, we may pass by insensible stages through ever-complicating forms of actions, with their accompanying signs of feeling and intelligence, until we reach the highest.

Even apart from the evidence derived from the ascending grades of animals up from *zoophytes*, as they are significantly named, it needs only to observe the evolution of a single animal to see that there does not exist any break or chasm between the life which shows no mind and the life which shows mind. The yelk of an egg which the cook has just broken, not only yields no sign of mind, but yields no sign of life. It does not respond to a stimulus as much even as many plants do. Had the egg, instead of being broken by the cook, been left under the hen for a certain time, the yelk would have passed by infinitesimal gradations through a series of forms ending in the chick; and by similarly infinitesimal gradations would have arisen those functions which end in the chick breaking its shell; and which, when it gets out, show themselves in running about, distinguishing and picking up food, and squeaking if hurt. When did the feeling begin? and how did there come into existence that power of perception which the chick's actions show? Should it be objected that the chick's actions are mainly automatic, I will not dwell on the fact that, though they are largely so, the chick manifestly has feeling and therefore consciousness; but I will accept the objection, and propose that instead we take the human being. The course of development before birth is just of the same general kind; and similarly, at a certain stage, begins to be accompanied by reflex movements. At birth there is displayed an amount of mind certainly not greater than that of the chick: there is no power of running from danger—no power of distinguishing and picking up food. If we say the chick is unintelligent, we must certainly say the infant is unintelligent. And yet from the unintelligence of the infant to the intelligence of the adult, there is an advance by steps so small that on no day is the amount of mind shown, appreciably different from that shown on preceding and succeeding days.

Thus the tacit assumption that there exists a break, is not simply gratuitous, but is negatived by the most obvious facts.

Certain of the words and phrases used in explaining that particular part of the Doctrine of Evolution which deals with the origin of species, are commented upon by Mr. Martineau as having implications justifying his view. Let us consider his comments.

He says that *competition* is not an "original power, which can of itself do anything;" further, that "it cannot act except in the presence of some *possibility of a better or worse;*" and that this "possibility of a better or worse" implies a "world pre-arranged for progress," "a directing Will intent upon the good." Had Mr. Martineau looked more closely into the matter, he would have found that, though the words and phrases he quotes are used for convenience, the conceptions they imply are not at all essential to the doctrine. Under its rigorously-scientific form, the doctrine is expressible in purely-physical terms, which neither imply competition nor imply better and worse.*

Beyond this indirect mistake there is a direct mistake. Mr. Martineau speaks of the "survivorship of the better," as though that were the statement of the law; and then adds that the alleged result cannot be inferred "except on the assumption that whatever is *better* is *stronger* too." But the words he here uses are his own words, not the words of those he opposes. The law is the survival of the *fittest*. Probably, in substituting "better" for "fittest," Mr. Martineau did not suppose that he was changing the meaning; though I dare say he perceived that the meaning of the word "fittest" did not suit his argument so well. Had he examined the facts, he would have found that the law is not the survival of the "better" or the "stronger," if we give to those words any thing like their ordinary meanings. It is the survival of those which are constitutionally fittest to thrive under the conditions in which they are placed; and very often that which, humanly speaking, is inferiority, causes the survival. Superiority, whether in size, strength, activity, or sagacity, is, other things equal, at the cost of diminished fertility; and where the life led by a species does not demand these higher attributes, the species profits by decrease of them, and accompanying increase of fertility, This is the reason why there occur so many cases of retrograde metamorphosis—this is the reason why parasites, internal and external, are so commonly degraded forms of higher types. Survival of the "better" does not cover these cases, though survival of the "fittest" does; and as I am responsible for the phrase, I suppose I am competent to say that the word "fittest" was chosen for this reason. When it is remembered that these cases outnumber all others—

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* *Principles of Biology*, §§ 159—168.

that there are more species of parasites than there are species of all other animals put together—it will be seen that the expression "survivorship of the better" is wholly inappropriate, and the argument Mr. Martineau bases upon it quite untenable. Indeed, if, in place of those adjustments of the human sense-organs, which he so eloquently describes as implying pre-arrangement, Mr. Martineau had described the countless elaborate appliances which enable parasites to torture animals immeasurably superior to them, and which, from his point of view, no less imply pre-arrangement, I think the notes of admiration which end his descriptions would not have seemed to him so appropriate.

One more word there is from the intrinsic meaning of which Mr. Martineau deduces what appears a powerful argument—the word *Evolution* itself. He says:—

> "It means, to unfold from within; and it is taken from the history of the seed or embryo of living natures. And what is the seed but a casket of pre-arranged futurities, with its whole contents *prospective*, settled to be what they are by reference to ends still in the distance?"

Now, this criticism would have been very much to the point did the word Evolution truly express the process it names. If this process, as scientifically defined, really involved that conception which the word evolution was originally designed to convey, the implications would be those Mr. Martineau alleges. But, unfortunately for him, the word, having been in possession of the field before the process was understood, has been adopted merely because displacing it by another word seemed impracticable. And this adoption of it has been joined with a caution against misunderstandings arising from its unfitness. Here is a part of the caution:—"Evolution has other meanings, some of which are incongruous with, and some even directly opposed to, the meaning here given to it. . . . The antithetical word, Involution, would much more truly express the nature of the process; and would, indeed, describe better the secondary characters of the process which we shall have to deal with presently."* So that the meanings which the word involves, and which Mr. Martineau regards as fatal to the hypothesis, are already repudiated as not belonging to the hypothesis.

And now, having dealt with the essential objections raised by Mr. Martineau to the Hypothesis of Evolution as it is presented under that purely scientific form which generalizes the process of things, firstly as observed and secondly as inferred from certain ultimate principles, let me go on to examine that form of the Hypothesis which he propounds—Evolution as determined by Mind and Will—Evolution as pre-arranged by a Divine Actor. For Mr. Martineau apparently abandons the primitive theory of creation by "fiat of Almighty Will", and also the theory of creation by manufacture—by "a contriving and adapting power," and seems to believe in evolution: requiring only that "an originating Mind" shall be taken as its antecedent. Let us ask, first, in what relation Mr. Martineau conceives the "originating Mind" to stand to the evolving Universe. From some passages it is inferable that he considers the "presence of mind" to be everywhere needful. He says:—

> "It is impossible to work the theory of Evolution upwards from the bottom. If all force is to be conceived as One, its type must be looked for in the highest and all-comprehending term; and Mind must be conceived as there, and as divesting itself of some speciality at each step of its descent to a lower stratum of law, till represented at the base under the guise of simple Dynamics."

This seems to be an unmistakable assertion that, wherever Evolution is going on, Mind is then and there behind it. At the close of the argument, however, a quite different conception is implied. Mr. Martineau says:—

> "If the Divine Idea will not retire at the bidding of our speculative science, but retains its place, it is natural to ask, What is its relation to

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* *First Principles*, second edition, § 97.

the series of so-called Forces in the world? But the question is too large and deep to be answered here. Let it suffice to say, that there need not be any *overruling* of these forces by the Will of God, so that the supernatural should disturb the natural; or any *supplementing* of them, so that He should fill up their deficiencies. Rather is His thought related to them as, in man, the mental force is related to all below it."

It would take too much space to deal fully with the various questions which this last passage raises. There is the question—Whence come these "Forces," spoken of as separate from the "Will of God"—did they pre-exist? Then what becomes of the Divine Power? Do they exist by the Divine Will? Then what kind of nature is that by which they act apart from the Divine Will? Again, there is the question—How do these deputy-forces co-operate in each particular phenomenon, if the presiding Will is not there present to control them? Either an organ which develops into fitness for its function, develops by the co-operation of these forces under the direction of Mind then present, or it so develops in the absence of Mind. If it develops in the absence of Mind, the hypothesis is given up; and if the "originating Mind" is required to be then and there present, we must suppose a particular providence to be present in each particular organ of each particular creature throughout the universe. Once more there is the question—If "His thought is related to them [these Forces] as, in Man, the mental force is related to all below it," how can "His thought" be regarded as the cause of Evolution? In man the mental force is related to the forces below it neither as a creator of them nor as a regulator of them, save in a very limited way: the greater part of the forces present in man, both structural and functional, defy the mental force absolutely. Nay, more, it needs but to injure a nerve to see that the power of the mental force over the physical forces is dependent on conditions which are themselves physical; and one who takes morphia in mistake for magnesia, discovers that the power of the physical forces over the mental is *un*conditioned by any thing mental.

Not dwelling on these questions, however, I will merely draw attention to the entire incongruity of this conception with the previous conception which I have quoted. Assuming that, when the choice is pressed on him, Mr. Martineau will choose the first, which alone has any thing like defensibility, let us go on to ask how far Evolution is made more comprehensible by postulating Mind, universally immanent, as its cause.

In metaphysical controversy, many of the propositions propounded and accepted as quite believable, are absolutely inconceivable. There is a perpetual confusing of actual ideas with what are nothing but pseud-ideas. No distinction is made between propositions that contain real thoughts, and propositions that are only the forms of thoughts. A thinkable proposition is one of which *the two terms can be brought together in consciousness under the relation said to exist between them.* But very often, when the subject of a proposition has been thought of as something known, and when the predicate has been thought of as something known, and when the relation alleged between them has been thought of as a known relation, it is supposed that the proposition itself has been thought. The thinking separately of the elements of a proposition is mistaken for the thinking of them in the combination which the proposition affirms. And hence it continually happens that propositions which cannot be rendered into thought at all, are supposed to be not only thought but believed. The proposition that Evolution is caused by Mind is one of this nature. The two terms are separately intelligible; but they can be regarded in the relation of effect and cause only so long as no attempt is made to put them together in this relation.

The only thing which any one knows as Mind is the series of his own states of consciousness; and if he thinks of any mind other than his own, he can think of it only in terms derived from his own. If I am asked to frame a notion of Mind divested of all those structural traits under which alone I am conscious of mind in myself, I cannot do it. I know nothing of thought save as carried on in ideas originally traceable to the effects wrought by objects and forces on me. A mental act is an unintelligible phrase if I am not to regard it as an act in which states of consciousness are severally known as like other states in the series that has gone by, and in which the relations between them are severally known as like past relations in the series. If, then, I have to conceive Evolution as caused by an "originating Mind," I must conceive this Mind as having attributes akin to those of the only mind I know, and without which I cannot conceive Mind at all.

I will not dwell on the many incongruities hence resulting, by asking how the "originating Mind" is to be thought of as having states produced by things objective to it; as discriminating among these states, and classing them as like and unlike; and as preferring one objective result to another. I will simply ask—What happens if we ascribe to the "originating Mind" the character absolutely essential to the conception of Mind, that it consists of a series of states of consciousness? Put a series of states of consciousness as cause, and the evolving Universe as effect, and then endeavor to see the last as flowing from the first. I find it possible to imagine in some dim way a series of states of consciousness serving as antecedent to any one of the movements I see going on; for my own states of consciousness are often indirectly the antecedents to such movements. But how if I attempt to think of such a series as antecedent to *all* actions throughout the Universe—to the motions of the multitudinous stars through space, to the revolutions of all their planets round them, to the gyrations of all these planets on their axes, to the infinitely-multiplied physical processes going on in each of these suns and planets? I cannot think of a single series of states of consciousness as causing even the relatively small group of actions going on over the Earth's surface. I cannot think of it even as antecedent to all the various winds and the dissolving clouds they bear, to the currents of all the rivers, and the grinding actions of all the glaciers; still less can I think of it as antecedent to the infinity of processes simultaneously going on in all the plants that cover the globe, from scattered polar lichens to crowded tropical palms, and in all the millions of quadrupeds that roam among them, and the millions of millions of insects that buzz about them. Even to a single small set of these multitudinous terrestrial changes, I cannot conceive as antecedent a single series of states of consciousness—cannot, for instance, think of it as causing the hundred thousand breakers that are at this instant curling over on the shores of England. How, then, is it possible for me to conceive an "originating Mind," which I must represent to myself as a *single* series of states of consciousness, working the infinitely-multiplied sets of changes *simultaneously* going on in worlds too numerous to count, dispersed throughout a space that baffles imagination?

If, to account for this infinitude of physical changes everywhere going on, "Mind must be conceived as there" "under the guise of simple Dynamics," then the reply is that, to be so conceived, Mind must be divested of all attributes by which it is distinguished; and that, when thus divested of its distinguishing attributes, the conception disappears—the word Mind stands for a blank. If Mr. Martineau takes refuge in the entirely different and, as it seems to me, incongruous hypothesis of something like a plurality of minds—if he accepts, as he seems to do, the doctrine that you cannot explain Evolution "unless among your primordial elements you scatter already the *germs* of Mind as well as the inferior elements"—if the insuperable difficulties I have just pointed out are to be met by assuming a local series of states of consciousness for each phenomenon, derived from his own. If I am asked to frame a notion of Mind divested of all those structural traits under which alone I am conscious of mind in myself, I cannot do it. I know nothing of thought save as carried on in ideas originally traceable to the effects wrought by objects and forces on me. A mental act is an unintelligible phrase if I am not to regard it as an act in which states of consciousness are severally known as like other states in the series that has gone by, and in which the relations between them are severally known as like past relations in the series. If, then, I have to conceive Evolution as caused by an "originating Mind," I must conceive this Mind as having attributes akin to those of the only mind I know, and without which I cannot conceive Mind at all.

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## THE FACTORS OF ORGANIC EVOLUTION.

[*First published in* The Nineteenth Century, *for April and May*, 1886.]

### I.

WITHIN the recollection of men now in middle life, opinion concerning the derivation of animals and plants was in a chaotic state. Among the unthinking there was tacit belief in creation by miracle, which formed an essential part of the creed of Christendom; and among the thinking there were two parties, each of which held an indefensible hypothesis. Immensely the larger of these parties, including nearly all whose scientific culture gave weight to their judgments, though not accepting literally the theologically-orthodox doctrine, made a compromise between that doctrine and the doctrines which geologists had established; while opposed to them were some, mostly having no authority in science, who held a doctrine which was heterodox both theologically and scientifically. Professor Huxley, in his lecture on "The Coming of Age of the Origin of Species," remarks concerning the first of these parties as follows:—

> "One-and-twenty years ago, in spite of the work commenced by Hutton and continued with rare skill and patience by Lyell, the dominant view of the past history of the earth was catastrophic. Great and sudden physical revolutions, wholesale creations and extinctions of living beings, were the ordinary machinery of the geological epic brought into fashion by the misapplied genius of Cuvier. It was gravely maintained and taught that the end of every geological epoch was signalised by a cataclysm, by which every living being on the globe was swept away, to be replaced by a brand-new creation when the world returned to quiescence. A scheme of nature which

> appeared to be modelled on the likeness of a succession of rubbers of whist, at the end of each of which the players upset the table and called for a new pack, did not seem to shock anybody.
>
> I may be wrong, but I doubt if, at the present time, there is a single responsible representative of these opinions left. The progress of scientific geology has elevated the fundament principle of uniformitarianism, that the explanation of the past is to be sought in the study of the present, into the position of an axiom; and the wild speculations of the catastrophists, to which we all listened with respect a quarter of a century ago, would hardly find a single patient hearer at the present day."

Of the party above referred to as not satisfied with this conception described by Professor Huxley, there were two classes. The great majority were admirers of the *Vestiges of the Natural History of Creation*—a work which, while it sought to show that organic evolution has taken place, contended that the cause of organic evolution, is "an impulse" supernaturally "imparted to the forms of life, advancing them, . . . through grades of organization." Being nearly all very inadequately acquainted with the facts, those who accepted the view set forth in the *Vestiges* were ridiculed by the well-instructed for being satisfied with evidence, much of which was either invalid or easily cancelled by counter-evidence, and at the same time they exposed themselves to the ridicule of the more philosophical for being content with a supposed explanation which was in reality no explanation: the alleged "impulse" to advance giving us no more help in understanding the facts than does Nature's alleged "abhorrence of a vacuum" help us to understand the ascent of water in a pump. The remnant, forming the second of these classes, was very small. While rejecting this mere verbal solution, which both Dr. Erasmus Darwin and Lamarck had shadowed forth in other language, there were some few who, rejecting also the hypothesis indicated by both Dr. Darwin and Lamarck, that the promptings of desires or wants produced growths of the parts subserving them, accepted the single *vera causa* assigned by these writers—the modification of structures resulting from modification of functions. They recognized as the sole process in organic development, the adaptation of parts and powers consequent on the effects of use and disuse—that continual moulding and re-moulding of organisms to suit their circumstances, which is brought about by direct converse with such circumstances.

But while this cause accepted by these few is a true cause, since unquestionably during the life of the individual organism changes of function produce changes of structure; and while it is a tenable hypothesis that changes of structure so produced are inheritable; yet it was manifest to those not prepossessed, that this cause cannot with reason be assigned for the greater part of the facts. Though in plants there are some characters which may not irrationally be ascribed to the direct effects of modified functions consequent on modified circumstances, yet the majority of the traits presented by plants are not to be thus explained. It is impossible that the thorns by which a briar is in large measure defended against browsing animals, can have been developed and moulded by the continuous exercise of their protective actions; for in the first place, the great majority of the thorns are never touched at all, and, in the second place, we have no ground whatever for supposing that those which are touched are thereby made to grow, and to take those shapes which render them efficient. Plants which are rendered uneatable by the thick woolly coatings of their leaves, cannot have had these coatings produced by any process of reaction against the action of enemies; for there is no imaginable reason why, if one part of a plant is eaten, the rest should thereafter begin to develop the hairs on its surface. By what direct effect of function on structure, can the shell of a nut have been evolved? Or how can those seeds which contain essential oils, rendering them unpalatable to birds, have been made to secrete such essential oils by these actions of birds which they restrain? Or how can the delicate plumes borne by some seeds, and giving the wind power to waft them to new stations, be due to any immediate influences of surrounding conditions? Clearly in these and in countless other cases, change of structure cannot have been directly caused by change of function. So is it with animals to a large extent, if not to the same extent. Though we have proof that by rough usage the dermal layer may be so excited as to produce a greatly thickened epidermal layer, sometimes quite horny; and though it is a feasible hypothesis that an effect of this kind persistently produced may be inherited; yet no such cause can explain the carapace of the turtle, the armour of the armadillo, or the imbricated covering of the manis. The skins of these animals are no more exposed to habitual hard usage than are those of animals covered by hair. The strange excrescences which distinguish the heads of the hornbills, cannot possibly have arisen from any reaction against the action of surrounding forces; for even were they clearly protective, there is no reason to suppose that the heads of these birds need protection more than the heads of other birds. If, led by the evidence that in animals the amount of covering is in some cases affected by the degree of exposure, it were admitted as imaginable that the development of feathers from preceding dermal growths had resulted from that extra nutrition caused by extra superficial circulation, we should still be without explanation of the structure of a feather. Nor should we have any clue to the specialities of feathers—the crests of various birds, the tails sometimes so enormous, the curiously placed plumes of the bird of paradise, &c., &c. Still more obviously impossible is it to explain as due to use or disuse the colours of animals. No direct adaptation to function could have produced the blue protuberances on a mandril's face, or the striped hide of a tiger, or the gorgeous plumage of a kingfisher, or the eyes in a peacock's tail, or the multitudinous patterns of insects' wings. One single case, that of a deer's horns, might alone have sufficed to show

how insufficient was the assigned cause. During their growth, a deer's horns are not used at all; and when, having been cleared of the dead skin and dried-up blood-vessels covering them, they are ready for use, they are nerveless and non-vascular, and hence are incapable of undergoing any changes of structure consequent on changes of function.

Of these few then, who rejected the belief described by Professor Huxley, and who, espousing the belief in a continuous evolution, had to account for this evolution, it must be said that though the cause assigned was a true cause, yet, even admitting that it operated through successive generations, it left unexplained the greater part of the facts. Having been myself one of these few, I look back with surprise at the way in which the facts which were congruous with the espoused view monopolized consciousness and kept out the facts which were incongruous with it—conspicuous though many of them were. The misjudgment was not unnatural. Finding it impossible to accept any doctrine which implied a breach in the uniform course of natural causation, and, by implication, accepting as unquestionable the origin and development of all organic forms by accumulated modifications naturally caused, that which appeared to explain certain classes of these modifications, was supposed to be capable of explaining the rest: the tendency being to assume that these would eventually be similarly accounted for, though it was not clear how.

Returning from this parenthethic remark, we are concerned here chiefly to remember that, as said at the outset, there existed thirty years ago, no tenable theory about the genesis of living things. Of the two alternative beliefs, neither would bear critical examination.

Out of this dead lock we were released—in large measure, though not I believe entirely—by the *Origin of Species*. That work brought into view a further factor; or rather, such factor, recognized as in operation by here and there an observer (as pointed out by Mr. Darwin in his introduction to the second edition), was by him for the first time seen to have played so immense a part in the genesis of plants and animals.

Though laying myself open to the charge of telling a thrice-told tale, I feel obliged here to indicate briefly the several great classes of facts which Mr. Darwin's hypothesis explains; because otherwise that which follows would scarcely be understood. And I feel the less hesitation in doing this because the hypothesis which it replaced, not very widely known at any time, has of late so completely dropped into the background, that the majority of readers are scarcely aware of its existence, and do not therefore understand the relation between Mr. Darwin's successful interpretation and the preceding unsuccessful attempt at interpretation. Of these classes of facts, four chief ones may be here distinguished.

In the first place, such adjustments as those exemplified above are made comprehensible. Though it is inconceivable that a structure like that of the pitcher-plant could have been produced by accumulated effects of function on structure; yet it is conceivable that successive selections of favourable variations might have produced it; and the like holds of the no less remarkable appliance of the Venus's Fly-trap, or the still more astonishing one of that water-plant by which infant-fish are captured. Though it is impossible to imagine how, by direct influence of increased use, such dermal appendages as a porcupine's quills could have been developed; yet, profiting as the members of a species otherwise defenceless might do by the stiffness of their hairs, rendering them unpleasant morsels to eat, it is a feasible supposition that from successive survivals of individuals thus defended in the greatest degrees, and the consequent growth in successive generations of hairs into bristles, bristles into spines, spines into quills (for all these are homologous), this change could have arisen. In like manner, the odd inflatable bag of the bladder-nosed seal, the curious fishing-rod with its worm-like appendage carried on the head of the *lophius* or angler, the spurs on the wings of certain birds, the weapons of the sword-fish and saw-fish, the wattles of fowls, and numberless such peculiar structures, though by no possibility explicable as due to effects of use or disuse, are explicable as resulting from natural selection operating in one or other way.

In the second place, while showing us how there have arisen countless modifications in the forms, structures, and colours of each part, Mr. Darwin has shown us how, by the establishment of favourable variations, there may arise new parts. Though the first step in the production of horns on the heads of various herbivorous animals, may have been the growth of callosities consequent on the habit of butting—such callosities thus functionally initiated being afterwards developed in the most advantageous ways by selection; yet no explanation can be thus given of the sudden appearance of a duplicate set of horns, as occasionally happens in sheep: an addition which, where it proved beneficial, might readily be made a permanent trait by natural selection. Again, the modifications which follow use and disuse can by no possibility account for changes in the numbers of vertebræ; but after recognizing spontaneous, or rather fortuitous, variation as a factor, we can see that where an additional vertebra hence resulting (as in some pigeons) proves beneficial, survival of the fittest may make it a constant character; and there may, by further like additions, be produced extremely long strings of vertebræ, such as snakes show us. Similarly with the mammary glands. It is not an unreasonable supposition that by the effects of greater or less function, inherited through successive generations, these may be enlarged or diminished in size; but it is out of the question to allege such a cause for changes in their numbers. There is no imaginable explanation of these save the establishment by inheritance of spontaneous variations, such as are known to occur in the human race.

So too, in the third place, with certain alterations in the connexions of parts. According to the greater or smaller demands made on this or that limb, the muscles moving it may be augmented or diminished in bulk; and, if there is inheritance of changes so wrought, the limb may, in course of generations, be rendered larger or smaller. But changes in the arrangements or attachments of muscles cannot be thus accounted for. It is found, especially at the extremities, that the relations of tendons to bones and to one another are not always the same. Variations in their modes of connexion may occasionally prove advantageous, and may thus become established. Here again, then, we have a class of structural changes to which Mr. Darwin's hypothesis gives us the key, and to which there is no other key.

Once more there are the phenomena of mimicry. Perhaps in a more striking way than any others, these show how traits which seem inexplicable are explicable as due to the more frequent survival of individuals that have varied in favourable ways. We are enabled to understand such marvellous simulations as those of the leaf-insect, those of beetles which "resemble glittering dew-drops upon the leaves;" those of caterpillars which, when asleep, stretch themselves out so as to look like twigs. And we are shown how there have arisen still more astonishing imitations—those of one insect by another. As Mr. Bates has proved, there are cases in which a species of butterfly, rendered so unpalatable to insectivorous birds by its disagreeable taste that they will not catch it, is simulated in its colours and markings by a species which is structurally quite different—so simulated that even a practised entomologist is liable to be deceived: the explanation being that an original slight resemblance, leading to occasional mistakes on the part of birds, was increased generation after generation by the more frequent escape of the most-like individuals, until the likeness became thus great.

But now, recognizing in full this process brought into clear view by Mr. Darwin, and traced out by him with so much care and skill, can we conclude that, taken alone, it accounts for organic evolution? Has the natural selection of favourable variations been the sole factor? On critically examining the evidence, we shall find reason to think that it by no means explains all that has to be explained. Omitting for the present any consideration of a factor which may be distinguished as primordial, it may be contended that the above-named factor alleged by Dr. Erasmus Darwin and by Lamarck, must be recognized as a co-operator. Utterly inadequate to explain the major part of the facts as is the hypothesis of the inheritance of functionally-produced modifications, yet there is a minor part of the facts, very extensive though less, which must be ascribed to this cause.

When discussing the question more than twenty years ago (*Principles of Biology*, § 166), I instanced the decreased size of the jaws in the civilized races of mankind, as a change not accounted for by the natural selection of favourable variations; since no one of the decrements by which, in thousands of years, this reduction has been effected, could have given to an individual in which it occurred, such advantage as would cause his survival, either through diminished cost of local nutrition or diminished weight to be carried. I did not then exclude, as I might have done, two other imaginable causes. It may be said that there is some organic correlation between increased size of brain and decreased size of jaw: Camper's doctrine of the facial angle being referred to in proof. But this argument may be met by pointing to the many examples of small-jawed people who are also small-brained, and by citing not infrequent cases of individuals remarkable for their mental powers, and at the same time distinguished by jaws not less than the average but greater. Again, if sexual selection be named as a possible cause, there is the reply that, even supposing such slight diminution of jaw as took place in a single generation to have been an attraction, yet the other incentives to choice on the part of men have been too many and great to allow this one to weigh in an adequate degree; while, during the greater portion of the period, choice on the part of women has scarcely operated: in earlier times they were stolen or bought, and in later times mostly coerced by parents. Thus, reconsideration of the facts does not show me the invalidity of the conclusion drawn, that this decrease in size of jaw can have had no other cause than continued inheritance of those diminutions consequent on diminutions of function, implied by the use of selected and well-prepared food. Here, however, my chief purpose is to add an instance showing, even more clearly, the connexion between change of function and change of structure. This instance, allied in nature to the other, is presented by those varieties, or rather sub-varieties, of dogs, which, having been household pets, and habitually fed on soft food, have not been called on to use their jaws in tearing and crunching, and have been but rarely allowed to use them in catching prey and in fighting. No inference can be drawn from the sizes of the jaws themselves, which, in these dogs, have probably been shortened mainly by selection. To get direct proof of the decrease of the muscles concerned in closing the jaws or biting, would require a series of observations very difficult to make. But it is not difficult to get indirect proof of this decrease by looking at the bony structures with which these muscles are connected. Examination of the skulls of sundry indoor dogs contained in the Museum of the College of Surgeons, proves the relative smallness of such parts. The only pug-dog's skull is that of an individual not perfectly adult; and though its traits are quite to the point they cannot with safety be taken as evidence. The skull of a toy-terrier has much restricted areas of insertion for the temporal muscles; has weak zygomatic arches; and has extremely small attachments for the masseter muscles. Still more significant is the evidence furnished by the skull of a King Charles's spaniel, which, if we allow three years to a generation, and bear in mind that the variety must have existed before Charles the Second's reign, we may assume belongs to something approaching to the hundredth generation of these household pets. The relative breadth between the outer surfaces of the zygomatic arches is conspicuously small; the narrowness of the temporal fossæ is also striking; the zygomata are very slender; the temporal muscles have left no marks whatever, either by limiting lines or by the character of the surfaces covered; and the places of attachment for the masseter muscles are very feebly developed. At the Museum of Natural History, among skulls of dogs there is one which, though unnamed, is shown by its small size and by its teeth, to have belonged to one variety or other of lap-dogs, and which has the same traits in an equal degree with the skull just described. Here, then, we have two if not three kinds of dogs which, similarly leading protected and pampered lives, show that in the course of generations the parts concerned in clenching the jaws have dwindled. To what cause must this decrease be ascribed? Certainly not to artificial selection; for most of the modifications named make no appreciable external signs: the width across the zygomata could alone be perceived. Neither can natural selection have had anything to do with it; for even were there any struggle for existence among such dogs, it cannot be contended that any advantage in the struggle could be gained by an individual in which a decrease took place. Economy of nutrition, too, is excluded. Abundantly fed as such dogs are, the constitutional tendency is to find places where excess of absorbed nutriment may be conveniently deposited, rather than to find places where some cutting down of the supplies is practicable. Nor again can there be alleged a possible correlation between these diminutions and that shortening of the jaws which has probably resulted from selection; for in the bull-dog, which has also relatively short jaws, these structures concerned in closing them are unusually large. Thus there remains as the only conceivable cause, the diminution of size which results from diminished use. The dwindling of a little-exercised part has, by inheritance, been made more and more marked in successive generations.

Difficulties of another class may next be exemplified—those which present themselves when we ask how there can be effected by the selection of favourable variations, such changes of structure as adapt an organism to some useful action in which many different parts co-operate. None can fail to see how a simple part may, in course of generations, be greatly enlarged, if each enlargement furthers, in some decided way, maintenance of the species. It is easy to understand, too, how a complex part, as an entire limb, may be increased as a whole by the simultaneous due increase of its co-operative parts; since if, while it is growing, the channels of supply bring to the limb an unusual quantity of blood, there will naturally result a proportionately greater size of all its components—bones, muscles, arteries, veins, &c. But though in cases like this, the co-operative parts forming some large complex part may be expected to vary together, nothing implies that they necessarily do so; and we have proof that in various cases, even when closely united, they do not do so. An example is furnished by those blind crabs named in the *Origin of Species* which inhabit certain dark caves of Kentucky, and which, though they have lost their eyes, have not lost the foot-stalks which carried their eyes. In describing the varieties which have been produced by pigeon-fanciers, Mr. Darwin notes the fact that along with changes in length of beak produced by selection, there have not gone proportionate changes in length of tongue. Take again the case of teeth and jaws. In mankind these have not varied together. During civilization the jaws have decreased, but the teeth have not decreased in proportion; and hence that prevalent crowding of them, often remedied in childhood by extraction of some, and in other cases causing that imperfect development which is followed by early decay. But the absence of proportionate variation in co-operative parts that are close together, and are even bound up in the same mass, is best seen in those varieties of dogs named above as illustrating the inherited effects of disuse. We see in them, as we see in the human race, that diminution in the jaws has not been accompanied by corresponding diminution in the teeth. In the catalogue of the College of Surgeons Museum, there is appended to the entry which identifies a Blenheim Spaniel's skull, the words—"the teeth are closely crowded together," and to the entry concerning the skull of a King Charles's Spaniel the words—"the teeth are closely packed, p. 3, is placed quite transversely to the axis of the skull." It is further noteworthy that in a case where there is no diminished use of the jaws, but where they have been shortened by selection, a like want of concomitant variation is manifested: the case being that of the bull-dog, in the upper jaw of which also, "the premolars . . . are excessively crowded, and placed obliquely or even transversely to the long axis of the skull."*

If, then, in cases where we can test it, we find no concomitant variation in co-operative parts that are near together—if we do not find it in parts which, though belonging to different tissues, are so closely united as teeth and jaws—if we do not find it even when the co-operative parts are not only closely united, but are formed out of the same tissue, like the crab's eye and its peduncle; what shall we say of co-operative parts which, besides being composed of different tissues, are remote from one another? Not only are we forbidden to assume that they vary together, but we are warranted in asserting that they can have no tendency to vary together. And what are the implications in cases where increase of a structure can be of no service unless there is concomitant increase in many distant structures, which have to join it in performing the action for which it is useful?

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* It is probable that this shortening has resulted not directly but indirectly, from the selection of individuals which were noted for tenacity of hold; for the bull-dog's peculiarity in this respect seems due to relative shortness of the upper jaw, giving the underhung structure which, involving retreat of the nostrils, enables the dog to continue breathing while holding.

As far back as 1864 (*Principles of Biology*, § 166) I named in illustration an animal carrying heavy horns—the extinct Irish elk; and indicated the many changes in bones, muscles, blood-vessels, nerves, composing the fore-part of the body, which would be required to make an increment of size in such horns advantageous. Here let me take another instance—that of the giraffe: an instance which I take partly because, in the sixth edition of the *Origin of Species*, issued in 1872, Mr. Darwin has referred to this animal when effectually disposing of certain arguments urged against his hypothesis. He there says:—

> "In order that an animal should acquire some structure specially and largely developed, it is almost indispensable that several other parts should be modified and co-adapted. Although every part of the body varies slightly, it does not follow that the necessary parts should always vary in the right direction and to the right degree" (p. 179).

And in the summary of the chapter, he remarks concerning the adjustments in the same quadruped, that "the prolonged use of all the parts together with inheritance will have aided in an important manner in their co-ordination" (p. 199): a remark probably having reference chiefly to the increased massiveness of the lower part of the neck; the increased size and strength of the thorax required to bear the additional burden; and the increased strength of the fore-legs required to carry the greater weight of both. But now I think that further consideration suggests the belief that the entailed modifications are much more numerous and remote than at first appears; and that the greater part of these are such as cannot be ascribed in any degree to the selection of favourable variations, but must be ascribed exclusively to the inherited effects of changed functions. Whoever has seen a giraffe gallop will long remember the sight as a ludicrous one. The reason for the strangeness of the motions is obvious. Though the fore limbs and the hind limbs differ so much in length, yet in galloping they have to keep pace—must take equal strides. The result is that at each stride, the angle which the hind limbs describe round their centre of motion is much larger than the angle described by the fore limbs. And beyond this, as an aid in equalizing the strides, the hind part of the back is at each stride bent very much downwards and forwards. Hence the hind-quarters appear to be doing nearly all the work. Now a moment's observation shows that the bones and muscles composing the hind-quarters of the giraffe, perform actions differing in one or other way and degree, from the actions performed by the homologous bones and muscles in a mammal of ordinary proportions, and from those in the ancestral mammal which gave origin to the giraffe. Each further stage of that growth which produced the large fore-quarters and neck, entailed some adapted change in sundry of the numerous parts composing the hind-quarters; since any failure in the adjustment of their respective strengths would entail some defect in speed and consequent loss of life when chased. It needs but to remember how, when continuing to walk with a blistered foot, the taking of steps in such a modified way as to diminish pressure on the sore point, soon produces aching of muscles which are called into unusual action, to see that over-straining of any one of the muscles of the giraffe's hind-quarters might quickly incapacitate the animal when putting out all its powers to escape; and to be a few yards behind others would cause death. Hence if we are debarred from assuming that co-operative parts vary together even when adjacent and closely united—if we are still more debarred from assuming that with increased length of fore-legs or of neck, there will go an appropriate change in any one muscle or bone in the hind-quarters; how entirely out of the question it is to assume that there will simultaneously take place the appropriate changes in *all* those many components of the hind-quarters which severally require re-adjustment. It is useless to reply that an increment of length in the fore-legs or neck might be retained and transmitted to posterity, waiting an appropriate variation in a particular bone or muscle in the hind-quarters, which, being made, would allow of a further increment. For besides the fact that until this secondary variation occurred the primary variation would be a disadvantage often fatal; and besides the fact that before such an appropriate secondary variation might be expected in the course of generations to occur, the primary variation would have died out; there is the fact that the appropriate variation of one bone or muscle in the hind-quarters would be useless without appropriate variations of all the rest—some in this way and some in that—a number of appropriate variations which it is impossible to suppose.

Nor is this all. Far more numerous appropriate variations would be indirectly necessitated. The immense change in the ratio of fore-quarters to hind-quarters would make requisite a corresponding change of ratio in the appliances carrying on the nutrition of the two. The entire vascular system, arterial and veinous, would have to undergo successive unbuildings and rebuildings to make its channels everywhere adequate to the local requirements;

since any want of adjustment in the blood-supply in this or that set of muscles, would entail incapacity, failure of speed, and loss of life. Moreover the nerves supplying the various sets of muscles would have to be proportionately changed; as well as the central nervous tracts from which they issued. Can we suppose that all these appropriate changes, too, would be step by step simultaneously made by fortunate spontaneous variations, occurring along with all the other fortunate spontaneous variations? Considering how immense must be the number of these required changes, added to the changes above enumerated, the chances against any adequate re-adjustments fortuitously arising must be infinity to one.

If the effects of use and disuse of parts are inheritable, then any change in the fore parts of the giraffe which affects the action of the hind limbs and back, will simultaneously cause, by the greater or less exercise of it, a re-moulding of each component in the hind limbs and back in a way adapted to the new demands; and generation after generation the entire structure of the hind-quarters will be progressively fitted to the changed structure of the fore-quarters: all the appliances for nutrition and innervation being at the same time progressively fitted to both. But in the absence of this inheritance of functionally-produced modifications, there is no seeing how the required re-adjustments can be made.

Yet a third class of difficulties stands in the way of the belief that the natural selection of useful variations is the sole factor of organic evolution. This class of difficulties, already pointed out in § 166 of the *Principles of Biology*, I cannot more clearly set forth than in the words there used. Hence I may perhaps be excused for here quoting them.

"Where the life is comparatively simple, or where surrounding circumstances render some one function supremely important, the survival of the fittest may readily bring about the appropriate structural change, without any aid from the transmission of functionally-acquired modifications. But in proportion as the life grows complex—in proportion as a healthy existence cannot be secured by a large endowment of some one power, but demands many powers; in the same proportion do there arise obstacles to the increase of any particular power, by "the preservation of favoured races in the struggle for life." As fast as the faculties are multiplied, so fast does it become possible for the several members of a species to have various kinds of superiorities over one another. While one saves its life by higher speed, another does the like by clearer vision, another by keener scent, another by quicker hearing, another by greater strength, another by unusual power of enduring cold or hunger, another by special sagacity, another by special timidity, another by special courage; and others by other bodily and mental attributes. Now it is unquestionably true that, other things equal, each of these attributes, giving its possessor an extra chance of life, is likely to be transmitted to posterity. But there seems no reason to suppose that it will be increased in subsequent generations by natural selection. That it may be thus increased, the individuals not possessing more than average endowments of it, must be more frequently killed off than individuals highly endowed with it; and this can happen only when the attribute is one of greater importance, for the time being, than most of the other attributes. If those members of the species which have but ordinary shares of it, nevertheless survive by virtue of other superiorities which they severally possess; then it is not easy to see how this particular attribute can be developed by natural selection in subsequent generations. The probability seems rather to be, that by gamogenesis, this extra endowment will, on the average, be diminished in posterity—just serving in the long run to compensate the deficient endowments of other individuals, whose special powers lie in other directions; and so to keep up the normal structure of the species. The working out of the process is here somewhat difficult to follow; but it appears to me that as fast as the number of bodily and mental faculties increases, and as fast as the maintenance of life comes to depend less on the amount of any one, and more on the combined action of all; so fast does the production of specialities of character by natural selection alone, become difficult. Particularly does this seem to be so with a species so multitudinous in its powers as mankind; and above all does it seem to be so with such of the human powers as have but minor shares in aiding the struggle for life—the æsthetic faculties, for example."

Dwelling for a moment on this last illustration of the class of difficulties described, let us ask how we are to interpret the development of the musical faculty. I will not enlarge on the family antecedents of the great composers. I will merely suggest the inquiry whether the greater powers possessed by Beethoven and Mozart, by Weber and Rossini, than by their fathers, were not due in larger measure to the inherited effects of daily exercise of the musical faculty by their fathers, than to inheritance, with increase, of spontaneous variations; and whether the diffused musical powers of the Bach clan, culminating in those of Johann Sebastian, did not result in part from constant practice; but I will raise the more general question—How came there that endowment of musical faculty which characterizes modern Europeans at large, as compared with their remote ancestors. The monotonous chants of low savages cannot be said to show any melodic inspiration; and it is not evident that an individual savage who had a little more musical perception than the rest, would derive any such advantage in the maintenance of life as would secure the spread of his superiority by inheritance of the variation. And then what are we to say of harmony? We cannot suppose that the appreciation of this, which is relatively modern, can have arisen by descent from the men in whom successive variations increased the appreciation of it—the composers and musical performers; for on the whole, these have been men whose worldly prosperity was not such as enabled them to rear many children inheriting their special traits. Even if we count the illegitimate ones, the survivors of these added to the survivors of the legitimate ones, can hardly be held to have yielded more than average numbers of descendants; and those who inherited their special traits have not often been thereby so aided in the struggle for existence as to further the spread of such traits. Rather the tendency seems to have been the reverse.

Since the above passage was written, I have found in the second volume of *Animals and Plants under Domestication*, a remark made by Mr. Darwin, practically implying that among creatures which depend for their lives on the efficiency of numerous powers, the increase of any one by the natural selection of a variation is necessarily difficult. Here it is.

"Finally, as indefinite and almost illimitable variability is the usual result of domestication and cultivation, with the same part or organ varying in different individuals in different or even in directly opposite ways; and as the same variation, if strongly pronounced, usually recurs only after long intervals of time, any particular variation would generally be lost by crossing, reversion, and the accidental destruction of the varying individuals, unless carefully preserved by man."—Vol. ii, 292.

Remembering that mankind, subject as they are to this domestication and cultivation, are not, like domesticated animals, under an agency which picks out and preserves particular variations; it results that there must usually be among them, under the influence of natural selection alone, a continual disappearance of any useful variations of particular faculties which may arise. Only in cases of variations which are specially preservative, as for example, great cunning during a relatively barbarous state, can we expect increase from natural selection alone. We cannot suppose that minor traits, exemplified among others by the æsthetic perceptions, can have been evolved by natural selection. But if there is inheritance of functionally-produced modifications of structure, evolution of such minor traits is no longer inexplicable.

Two remarks made by Mr. Darwin have implications from which the same general conclusion must, I think, be drawn. Speaking of the variability of animals and plants under domestication, he says:—

"Changes of any kind in the conditions of life, even extremely slight changes, often suffice to cause variability. . . Animals and plants continue to be variable for an immense period after their first domestication; . . . In the course of time they can be habituated to certain changes, so as to become less variable; . . . There is good evidence that the power of changed conditions accumulates; so that two, three, or more generations must be exposed to new conditions before any effect is visible. . . . Some variations are induced by the direct action of the surrounding conditions on the whole organization, or on certain parts alone, and other variations are induced indirectly through the reproductive system being affected in the same manner as is so common with organic beings when removed from their natural conditions."—(*Animals and Plants under Domestication*, vol. ii, 270.)

There are to be recognized two modes of this effect produced by changed conditions on the reproductive system, and consequently on offspring. Simple arrest of development is one. But beyond the variations of offspring arising from imperfectly developed reproductive systems in parents —variations which must be ordinarily in the nature of imperfections—there are others due to a changed balance of functions caused by changed conditions. The fact noted by Mr. Darwin in the above passage, "that the power of changed conditions accumulates; so that two, three, or more generations must be exposed to new conditions before any effect is visible," implies that during these generations there is going on some change of constitution consequent on the changed proportions and relations of the functions. I will not dwell on the implication, which seems tolerably clear, that this change must consist of such modifications of organs as adapt them to their changed functions; and that if the influence of changed conditions "accumulates," it must be through the inheritance of such modifications. Nor will I press the question—What is the nature of the effect registered in the reproductive elements, and which is subsequently manifested by variations?—Is it an effect entirely irrelevant to the new requirements of the variety? —Or is it an effect which makes the variety less fit for the new requirements?—Or is it an effect which makes it more fit for the new requirements? But not pressing these questions, it suffices to point out the necessary implication that changed functions of organs *do*, in some way or other, register themselves in changed proclivities of the reproductive elements. In face of these facts it cannot be denied that the modified action of a part produces an inheritable effect—be the nature of that effect what it may.

The second of the remarks above adverted to as made by Mr. Darwin, is contained in his sections dealing with correlated variations. In the *Origin of Species*, p. 114, he says—

> "The whole organization is so tied together during its growth and development, that when slight variations in any one part occur, and are accumulated through natural selection, other parts become modified."

And a parallel statement contained in *Animals and Plants under Domestication*, vol. ii, p. 320, runs thus—

> "Correlated variation is an important subject for us; for when one part is modified through continued selection, either by man or under nature, other parts of the organization will be unavoidably modified. From this correlation it apparently follows that, with our domesticated animals and plants, varieties rarely or never differ from each other by some single character alone."

By what process does a changed part modify other parts? By modifying their functions in some way or degree, seems the necessary answer. It is indeed, imaginable, that where the part changed is some dermal appendage which, becoming larger, has abstracted more of the needful material from the general stock, the effect may consist simply in diminishing the amount of this material available for other dermal appendages, leading to diminution of some or all of them, and may fail to affect in appreciable ways the rest of the organism: save perhaps the blood-vessels near the enlarged appendage. But where the part is an active one—a limb, or viscus, or any organ which constantly demands blood, produces waste matter, secretes, or absorbs—then all the other active organs become implicated in the change. The functions performed by them have to constitute a moving equilibrium; and the function of one cannot, by alteration of the structure performing it, be modified in degree or kind, without modifying the functions of the rest—some appreciably and others inappreciably, according to the directness or indirectness of their relations. Of such inter-dependent changes, the normal ones are naturally inconspicuous; but those which are partially or completely abnormal, sufficiently carry home the general truth. Thus, unusual cerebral excitement affects the excretion through the kidneys in quantity or quality or both. Strong emotions of disagreeable kinds check or arrest the flow of bile. A considerable obstacle to the circulation offered by some important structure in a diseased or disordered state, throwing more strain upon the heart, causes hypertrophy of its muscular walls; and this change which is, so far as concerns the primary evil, a remedial one, often entails mischiefs in other organs. "Apoplexy and palsy, in a scarcely credible number of cases, are directly dependent on hypertropic enlargement of the heart." And in other cases, asthma, dropsy, and epilepsy are caused. Now if a result of this inter-dependence as seen in the individual organism, is that a local modification of one part produces, by changing their functions, correlative modifications of other parts, then the question here to be put is—Are these correlative modifications, when of a kind falling within normal limits, inheritable or not. If they are inheritable, then the fact stated by Mr. Darwin that "when one part is modified through continued selection," "other parts of the organization will be unavoidably modified" is perfectly intelligible: these entailed secondary modifications are transmitted *pari passu* with the successive modifications produced by selection. But what if they are not inheritable? Then these secondary modifications caused in the individual, not being transmitted to descendants, the descendants must commence life with organizations out of balance, and with each increment of change in the part affected by selection, their organizations must get more out of balance—must have a larger and larger amounts of re-organization to be made during their lives. Hence the constitution of the variety must become more and more unworkable.

The only imaginable alternative is that the re-adjustments are effected in course of time by natural selection. But, in the first place, as we find no proof of concomitant variation among directly co-operative parts which are closely united, there cannot be assumed any concomitant variation among parts which are both indirectly co-operative and far from one another. And, in the second place, before all the many required re-adjustments could be made, the variety would die out from defective constitution. Even were there no such difficulty, we should still have to entertain a strange group of propositions, which would stand as follows:—1. Change in one part entails, by reaction on the organism, changes, in other parts, the functions of which are necessarily changed. 2. Such changes worked in the individual, affect, in some way, the reproductive elements: these being found to evolve unusual structures when the constitutional balance has been continuously disturbed. 3. But the changes in the reproductive elements thus caused, are not such as represent these functionally-produced changes: the modifications conveyed to offspring are irrelevant to these various modifications functionally produced in the organs of the parents. 4. Nevertheless, while the balance of functions cannot be re-established through inheritance of the effects of disturbed functions on structures, wrought throughout the individual organism; it can be re-established by the inheritance of fortuitous variations which occur in all the affected organs without reference to these changes of function.

Now without saying that acceptance of this group of propositions is impossible, we may certainly say that it is not easy.

"But where are the direct proofs that inheritance of functionally-produced modifications takes place?" is a question which will be put by those who have committed themselves to the current exclusive interpretation. "Grant that there are difficulties; still, before the transmitted effects of use and disuse can be legitimately assigned in explanation of them, we must have good evidence that the effects of use and disuse *are* transmitted."

Before dealing directly with this demurrer, let me deal with it indirectly, by pointing out that the lack of recognized evidence may be accounted for without assuming that there is not plenty of it. Inattention and reluctant attention lead to the ignoring of facts which really exist in abundance; as is well illustrated in the case of pre-historic implements. Biassed by the current belief that no traces of man were to be found on the Earth's surface, save in certain superficial formations of very recent date, geologists and anthropologists not only neglected to seek such traces, but for a long time continued to pooh-pooh those who said they had found them. When M. Boucher de Perthes at length succeeded in drawing the eyes of scientific men to the flint implements discovered by him in the quarternary deposits of the Somme valley; and when geologists and anthropologists had thus been convinced that evidences of human existence were to be found in formations of considerable age, and thereafter began to search for them; they found plenty of them all over the world. Or again, to take an instance closely germane to the matter, we may recall the fact that the contemptuous attitude towards the hypothesis of organic evolution which naturalists in general maintained before the publication of Mr. Darwin's work, prevented them from seeing the multitudinous facts by which it is supported. Similarly, it is very possible that their alienation from the belief that there is a transmission of those changes of structure which are produced by changes of action, makes naturalists slight the evidence which supports that belief and refuse to occupy themselves in seeking further evidence.

If it be asked how it happens that there have been recorded multitudinous instances of variations fortuitously arising and re-appearing in offspring, while there have not been recorded instances of the transmission of changes functionally produced, there are three replies. The first is that changes of the one class are many of them conspicuous, while those of the other class are nearly all inconspicuous. If a child is born with six fingers, the anomaly is not simply obvious but so startling as to attract much notice; and if this child, growing up, has six-fingered descendents, everybody in the locality hears of it. A pigeon with specially-coloured feathers, or one distinguished by a broadened and upraised tail, or by a protuberance of the neck, draws attention by its oddness; and if in its young the trait is repeated, occasionally with increase, the fact is remarked, and there follows the thought of establishing the peculiarity by selection. A lamb disabled from leaping by the shortness of its legs, could not fail to be observed; and the fact that its offspring were similarly short-legged, and had a consequent inability to get over fences, would inevitably become widely known. Similarly with plants. That this flower had an extra number of petals, that that was unusually symmetrical, and that another differed considerably in colour from the average of its kind, would be easily seen by an observant gardener; and the suspicion that such anomalies are inheritable having arisen, experiments leading to further proofs that they are so, would frequently be made. But it is not thus with functionally-produced modifications. The seats of these are in nearly all cases the muscular, osseous, and nervous systems, and the viscera—parts which are either entirely hidden or greatly obscured. Modification in a nervous centre is inaccessible to vision; bones may be considerably altered in size or shape without attention being drawn to them; and, covered with thick coats as are most of the animals open to continuous observation, the increases or decreases in muscles must be great before they become externally perceptible.

A further important difference between the two inquiries is that to ascertain whether a fortuitous variation is inheritable, needs merely a little attention to the selection of individuals and the observation of offspring; while to ascertain whether there is inheritance of a functionally-produced modification, it is requisite to make arrangements which demand the greater or smaller exercise of some part or parts; and it is difficult in many cases to find such arrangements, troublesome to maintain them even for one generation, and still more through successive generations.

Nor is this all. There exist stimuli to inquiry in the one case which do not exist in the other. The money-interest and the interest of the fancier, acting now separately and now together, have prompted multitudinous individuals to make experiments which have brought out clear evidence that fortuitous variations are inherited. The cattle-breeders who profit by producing certain shapes and qualities; the keepers of pet animals who take pride in the perfections of those they have bred; the florists, professional and amateur, who obtain new varieties and take prizes; form a body of men who furnish naturalists with countless of the required proofs. But there is no such body of men, led either by pecuniary interest or the interest of a hobby, to ascertain by experiments whether the effects of use and disuse are inheritable.

Thus, then, there are amply sufficient reasons why there is a great deal of direct evidence in the one case and but little in the other: such little being that which comes out incidentally. Let us look at what there is of it.

Considerable weight attaches to a fact which Brown-Séquard discovered, quite by accident, in the course of his researches. He found that certain artificially-produced lesions of the nervous system, so small even as a section of the sciatic nerve, left, after healing, an increasing excitability which ended in liability to epilepsy; and there afterwards came out the unlooked-for result that the offspring of guinea-pigs which had thus acquired an epileptic habit such that a pinch on the neck would produce a fit, inherited an epileptic habit of like kind. It has, indeed, been since alleged that guinea pigs tend to epilepsy, and that phenomena of the kind described, occur where there have been no antecedents like those in Brown-

Séquard's case. But considering the improbability that the phenomena observed by him happened to be nothing more than phenomena which occasionally arise naturally, we may, until there is good proof to the contrary, assign some value to his results.

Evidence not of this directly experimental kind, but nevertheless of considerable weight, is furnished by other nervous disorders. There is proof enough that insanity admits of being induced by circumstances which, in one or other way, derange the nervous functions—excesses of this or that kind; and no one questions the accepted belief that insanity is inheritable. Is it alleged that the insanity which is inheritable is that which spontaneously arises, and that the insanity which follows some chronic perversion of functions is not inheritable? This does not seem a very reasonable allegation; and until some warrant for it is forthcoming, we may fairly assume that there is here a further support for belief in the transmission of functionally-produced changes.

Moreover, I find among physicians the belief that nervous disorders of a less severe kind are inheritable. Men who have prostrated their nervous systems by prolonged overwork or in some other way, have children more or less prone to nervousness. It matters not what may be the form of inheritance—whether it be of a brain in some way imperfect, or of a deficient blood-supply; it is in any case the inheritance of functionally-modified structures.

Verification of the reasons above given for the paucity of this direct evidence, is yielded by contemplation of it; for it is observable that the cases named are cases which, from one or other cause, have thrust themselves on observation. They justify the suspicion that it is not because such cases are rare that many of them cannot be cited; but simply because they are mostly unobtrusive, and to be found only by that deliberate search which nobody makes. I say nobody, but I am wrong. Successful search has been made by one whose competence as an observer is beyond question, and whose testimony is less liable than that of all others to any bias towards the conclusion that such inheritance takes place. I refer to the author of the *Origin of Species.*

Now-a-days most naturalists are more Darwinian than Mr. Darwin himself. I do not mean that their beliefs in organic evolution are more decided; though I shall be supposed to mean this by the mass of readers, who identify Mr. Darwin's great contribution to the theory of organic evolution, with the theory of organic evolution itself, and even with the theory of evolution at large. But I mean that the particular factor which he first recognized as having played so immense a part in organic evolution, has come to be regarded by his followers as the sole factor, though it was not so regarded by him. It is true that he apparently rejected altogether the causal agencies alleged by earlier inquirers. In the Historical Sketch prefixed to the later editions of his *Origin of Species* (p. xiv, note), he writes :—"It is curious how largely my grandfather, Dr. Erasmus Darwin, anticipated the views and erroneous grounds of opinion of Lamarck in his 'Zoonomia' (vol. i, pp. 500-510), published in 1794." And since, among the views thus referred to, was the view that changes of structure in organisms arise by the inheritance of functionally-produced changes, Mr. Darwin seems, by the above sentence, to have implied his disbelief in such inheritance. But he did not mean to imply this; for his belief in it as a cause of evolution, if not an important cause, is proved by many passages in his works. In the first chapter of the *Origin of Species* (p. 8 of the sixth edition), he says respecting the inherited effects of habit, that "with animals the increased use or disuse of parts has had a more marked influence;" and he gives as instances the changed relative weights of the wing bones and leg bones of the wild duck and the domestic duck, "the great and inherited development of the udders in cows and goats," and the drooping ears of various domestic animals. Here are other passages taken from the latest edition of the work.

> "I think there can be no doubt that use in our domestic animals has strengthened and enlarged certain parts, and disuse diminished them; and that such modifications are inherited" (p. 108). [And on the following pages he gives five further examples of such effects.] "Habit in producing constitutional peculiarities and use in strengthening and disuse in weakening and diminishing organs, appear in many cases to have been potent in their effects" (p. 131). "When discussing special cases, Mr. Mivart passes over the effects of the increased use and disuse of parts, which I have always maintained to be highly important, and have treated in my 'Variation under Domestication' at greater length than, as I believe, any other writer" (p. 176). "Disuse, on the other hand, will account for the less developed condition of the whole inferior half of the body, including the lateral fins" (p. 188). "I may give another instance of a structure which apparently owes its origin exclusively to use or habit" (p. 188). "It appears probable that disuse has been the main agent in rendering organs rudimentary" (pp. 400—401). "On the whole, we may conclude that habit, or use and disuse, have, in some cases, played a considerable part in the modification of the constitution and structure; but that the effects have often been largely combined with, and sometimes overmastered by, the natural selection of innate variations" (p. 114).

In his subsequent work, *The Variation of Animals and Plants under Domestication*, where he goes into full detail, Mr. Darwin gives more numerous illustrations of the inherited effects of use and disuse. The following are some of the cases, quoted from volume i of the first edition.

> Treating of domesticated rabbits, he says:—"the want of exercise has apparently modified the proportional length of the limbs in comparison with the body" (p. 116). "We thus see that the most important and complicated organ [the brain] in the whole organization is subject to the law of decrease in size from disuse" (p. 129). He remarks that in birds of the oceanic islands "not persecuted by any enemies, the reduction of their wings has probably been caused by gradual disuse." After comparing one of these, the water-hen of Tristan d'Acunha, with the European water-hen, and showing that all the bones concerned in flight are smaller, he adds—"Hence in the skeleton of this natural species nearly the same changes have occurred, only carried a little further, as with our domestic ducks, and in this latter case I presume no one will dispute that they have resulted from the lessened use of the wings and the increased use of the legs" (pp. 286-7). "As with other long-domesticated animals, the instincts of the silk-moth have suffered. The

caterpillars, when placed on a mulberry-tree, often commit the strange mistake of devouring the base of the leaf on which they are feeding, and consequently fall down; but they are capable, according to M. Robinet, of again crawling up the trunk. Even this capacity sometimes fails, for M. Martins placed some caterpillars on a tree, and those which fell were not able to remount and perished of hunger; they were even incapable of passing from leaf to leaf" (p. 304).

Here are some instances of like meaning from volume ii.

"In many cases there is reason to believe that the lessened use of various organs has affected the corresponding parts in the offspring. But there is no good evidence that this ever follows in the course of a single generation. . . Our domestic fowls, ducks, and geese have almost lost, not only in the individual but in the race, their power of flight; for we do not see a chicken, when frightened, take flight like a young pheasant. . . . With domestic pigeons, the length of the sternum, the prominence of its crest, the length of the scapulæ and furcula, the length of the wings as measured from tip to tip of the radius, are all reduced relatively to the same parts in the wild pigeon." [After detailing kindred diminutions in fowls and ducks, Mr. Darwin adds] "The decreased weight and size of the bones, in the foregoing cases, is probably the indirect result of the reaction of the weakened muscles on the bones" (pp. 297-8). "Nathusius has shown that, with the improved races of the pig, the shortened legs and snout, the form of the articular condyles of the occiput, and the position of the jaws with the upper canine teeth projecting in a most anomalous manner in front of the lower canines, may be attributed to these parts not having been fully exercised. . . . These modifications of structure, which are all strictly inherited, characterise several improved breeds, so that they cannot have been derived from any single domestic or wild stock. With respect to cattle, Professor Tanner has remarked that the lungs and liver in the improved breeds 'are found to be considerably reduced in size when compared with those possessed by animals having perfect liberty;' . . . The cause of the reduced lungs in highly-bred animals which take little exercise is obvious" (pp. 299-300). [And on pp. 301, 302 and 303, he gives facts showing the effects of use and disuse in changing, among domestic animals, the characters of the ears, the lengths of the intestines, and, in various ways, the natures of the instincts.]

But Mr. Darwin's admission, or rather his assertion, that the inheritance of functionally-produced modifications has been a factor in organic evolution, is made clear not by these passages alone and by kindred ones. It is made clearer still by a passage in the preface to the second edition of his *Descent of Man*. He there protests against that current version of his views in which this factor makes no appearance. The passage is as follows.

> "I may take this opportunity of remarking that my critics frequently assume that I attribute all changes of corporeal structure and mental power exclusively to the natural selection of such variations as are often called spontaneous; whereas, even in the first edition of the 'Origin of Species,' I distinctly stated that great weight must be attributed to the inherited effects of use and disuse, with respect both to the body and mind."

Nor is this all. There is evidence that Mr. Darwin's belief in the efficiency of this factor, became stronger as he grew older and accumulated more evidence. The first of the extracts above given, taken from the sixth edition of the *Origin of Species*, runs thus :—

> "I think there can be no doubt that use in our domestic animals has strengthened and enlarged certain parts, and disuse diminished them; and that such modifications are inherited."

Now on turning to the first edition, p. 134, it will be found that instead of the words—"I think there can be no doubt," the words originally used were—"I think there can be *little* doubt." That this deliberate erasure of a qualifying word and substitution of a word implying unqualified belief, was due to a more decided recognition of a factor originally under-estimated, is clearly implied by the wording of the above-quoted passage from the preface to the *Descent of Man;* where he says that "*even* in the first edition of the 'Origin of Species,'" &c.: the implication being that much more in subsequent editions, and subsequent works, had he insisted on this factor. The change thus indicated is especially significant as having occurred at a time of life when the natural tendency is towards fixity of opinion.

During that earlier period when he was discovering the multitudinous cases in which his own hypothesis afforded solutions, and simultaneously observing how utterly futile in these multitudinous cases was the hypothesis propounded by his grandfather and Lamarck, Mr. Darwin was, not unnaturally, almost betrayed into the belief that the one is all-sufficient and the other inoperative. But in the mind of one so candid and ever open to more evidence, there naturally came a reaction. The inheritance of functionally-produced modifications, which, judging by the passage quoted above concerning the views of these earlier enquirers, would seem to have been at one time denied, but which as we have seen was always to some extent recognized, came to be recognized more and more, and deliberately included as a factor of importance.

Of this reaction displayed in the later writings of Mr. Darwin, let us now ask—Has it not to be carried further? Was the share in organic evolution which Mr. Darwin latterly assigned to the transmission of modifications caused by use and disuse, its due share? Consideration of the groups of evidences given above, will, I think, lead us to believe that its share has been much larger than he supposed even in his later days.

There is first the implication yielded by extensive classes of phenomena which remain inexplicable in the absence of this factor. If, as we see, co-operative parts do not vary together, even when few and close together, and may not therefore be assumed to do so when many and remote, we cannot account for those innumerable changes in organization which are implied when, for advantageous use of some modified part, many other parts which join it in action have to be modified.

Further, as increasing complexity of structure, accompanying increasing complexity of life, implies increasing number of faculties, of which each one conduces to preservation of self or descendants; and as the various individuals of a species, severally requiring something like the normal amounts of all these, may individually profit, here by an unusual amount of one, and there by an unusual amount of another; it follows that as the number of faculties becomes greater, it becomes more difficult for any one to be further developed by natural selection. Only where increase of some one is *predominantly* advantageous does the means seem adequate to the end. Especially in the case of powers which do not subserve self-preservation in appreciable degrees, does development by natural selection appear impracticable.

It is a fact recognized by Mr. Darwin, that where, by selection through successive generations, a part has been increased or decreased, its reaction upon other parts entails changes in them. This reaction is effected through the changes of function involved. If the changes of structure produced by such changes of function, are inheritable, then the re-adjustment of parts throughout the organism, taking place generation after generation, maintains an approximate balance; but if not, then generation after generation the organism must get more and more out of gear, and tend to become unworkable.

Further, as it is proved that change in the balance of functions registers its effects on the reproductive elements, we have to choose between the alternatives that the registered effects are irrelevant to the particular modifications which the organism has undergone, or that they are such as tend to produce repetitions of these modifications. The last of these alternatives makes the facts comprehensible; but the first of them not only leaves us with several unsolved problems, but is incongruous with the general truth that by reproduction, ancestral traits, down to minute details, are transmitted.

Though, in the absence of pecuniary interests and the interests in hobbies, no such special experiments as those which have established the inheritance of fortuitous variations have been made to ascertain whether functionally-produced modifications are inherited; yet certain apparent instances of such inheritance have forced themselves on observation without being sought for. In addition to other indications of a less conspicuous kind, is the one I have given above—the fact that the apparatus for tearing and mastication has decreased with decrease of its function, alike in civilized man and in some varieties of dogs which lead protected and pampered lives. Of the numerous cases named by Mr. Darwin, it is observable that they are yielded not by one class of parts only, but by most if not all classes—by the dermal system, the muscular system, the osseous system, the nervous system, the viscera; and that among parts liable to be functionally modified, the most numerous observed cases of inheritance are furnished by those which admit of preservation and easy comparison—the bones: these cases, moreover, being specially significant as showing how, in sundry unallied species, parallel changes of structure have occurred along with parallel changes of habit.

What, then, shall we say of the general implication? Are we to stop short with the admission that inheritance of functionally-produced modifications takes place only in cases in which there is evidence of it? May we properly assume that these many instances of changes of structure caused by changes of function, occurring in various tissues and various organs, are merely special and exceptional instances having no general significance? Shall we suppose that though the evidence which already exists has come to light without aid from a body of inquirers, there would be no great increase were due attention devoted to the collection of evidence? This is, I think, not a reasonable supposition. To me the *ensemble* of the facts suggests the belief, scarcely to be resisted, that the inheritance of functionally-produced modifications takes place universally. Looking at physiological phenomena as conforming to physical principles, it is difficult to conceive that a changed play of organic forces which in many cases of different kinds produces an inherited change of structure, does not do this in all cases. The implication, very strong I think, is that the action of every organ produces on it a reaction which, usually not altering its rate of nutrition, sometimes leaves it with diminished nutrition consequent on diminished action, and at other times increases its nutrition in proportion to its increased action; that while generating a modified *consensus* of functions and of structures, the activities are at the same time impressing this modified *consensus* on the sperm-cells and germ-cells whence future individuals are to be produced; and that in ways mostly too small to be identified, but occasionally in more conspicuous ways and in the course of generations, the resulting modifications of one or other kind show themselves. Further, it seems to me that as there are certain extensive classes of phenomena which are inexplicable if we assume the inheritance of fortuitous variations to be the sole factor, but which become at once explicable if we admit the inheritance of functionally-produced changes, we are justified in concluding that this inheritance of functionally-produced changes has been not simply a co-operating factor in organic evolution, but has been a co-operating factor without which organic evolution, in its higher forms at any rate, could never have taken place.

Be this or be it not a warrantable conclusion, there is, I think, good reason for a provisional acceptance of the hypothesis that the effects of use and disuse are inheritable; and for a methodic pursuit of inquiries with the view of either establishing it or disproving it. It seems scarcely reasonable to accept without clear demonstration, the belief that while a trivial difference of structure arising spontaneously is transmissible, a massive difference of structure, maintained generation after generation by change of function, leaves no trace in posterity. Considering that unquestionably the modification of structure by function is a *vera causa*, in so far as concerns the individual; and considering the number of facts which so competent an observer as Mr. Darwin regarded as evidence that transmission of such modifications takes place in particular cases; the hypothesis that such transmission takes place in conformity with a general law, holding of all active structures, should, I think, be regarded as at least a good working hypothesis.

But now supposing the broad conclusion above drawn to be granted—supposing all to agree that from the beginning, along with inheritance of useful variations fortuitously arising, there has been inheritance of effects produced by use and disuse; do there remain no classes of organic phenomena unaccounted for? To this question I think it must be replied that there do remain classes of organic phenomena unaccounted for. It may, I believe, be shown that certain cardinal traits of animals and plants at large are still unexplained; and that a further factor must be recognized. To show this, however, will require another paper.

## II.

Ask a plumber who is repairing your pump, how the water is raised in it, and he replies—"By suction." Recalling the ability which he has to suck up water into his mouth through a tube, he is certain that he understands the pump's action. To inquire what he means by suction, seems to him absurd. He says you know as well as he does, what he means; and he cannot see that there is any need for asking how it happens that the water rises in the tube when he strains his mouth in a particular way. To the question why the pump, acting by suction, will not make the water rise above 32 feet, and practically not so much, he can give no answer; but this does not shake his confidence in his explanation.

On the other hand an inquirer who insists
what suction is, may obtain from the
which give him clear ideas, not o
many other things. He learns

things around, there is an atmospheric pressure amounting to about 15 pounds on the square inch: 15 pounds being the average weight of a column of air having a square inch for its base and extending upwards from the sea-level to the limit of the Earth's atmosphere. He is made to observe that when he puts one end of a tube into water and the other end into his mouth, and then draws back his tongue, so leaving a vacant space, two things happen. One is that the pressure of air outside his cheeks, no longer balanced by an equal pressure of air inside, thrusts his cheeks inwards; and the other is that the pressure of air on the surface of the water, no longer balanced by an equal pressure of air within the tube and his mouth (into which part of the air from the tube has gone) the water is forced up the tube in consequence of the unequal pressure. Once understanding thus the nature of the so-called suction, he sees how it happens that when the plunger of the pump is raised and relieves from atmospheric pressure the water below it, the atmospheric pressure on the water in the well, not being balanced by that on the water in the tube, forces the water higher up the tube, so that it follows the plunger. And now he sees why the water cannot be raised beyond the theoretic limit of 32 feet: a limit made much lower in practice by imperfections in the apparatus. For if, simplifying the conception, he supposes the tube of the pump to be a square inch in section, then the atmospheric pressure of 15 pounds per square inch on the water in the well, can raise the water in the tube to such height only that the entire column of it weighs 15 pounds. Having been thus enlightened about the pump's action, the action of a barometer becomes intelligible. He perceives how, under the conditions established, the weight of the column of mercury balances that of an atmospheric column of equal diameter; and how, as the weight of the atmospheric column varies, there is a corresponding variation in the weight of the mercurial column,—shown by change of height. Moreover, having previously supposed that he understood the ascent of a balloon when he ascribed it to relative lightness, he now sees that he did not truly understand it. For he did not recognize it as a result of that upward pressure caused by the difference between the weight of the mass formed by the gas in the balloon *plus* the cylindrical column of air extending above it to the limit of the atmosphere, and the weight of a similar cylindrical column of air extending down to the under surface of the balloon: this difference of weight causing an equivalent upward pressure on the under surface.

Why do I introduce these familiar truths so entirely irrelevant to my subject? I do it to show, in the first place, the contrast between a vague conception of a cause and a distinct conception of it; or rather, the contrast between that conception of a cause which results when it is simply classed with some other or others which familiarity makes us think we understand, and that conception of a cause which results when it is represented in terms of definite physical forces admitting of measurement. And I do it to show, in the second place, that when we insist on resolving a verbally-intelligible cause into its actual factors, we get not only a clear solution of the problem before us, but we find that the way is opened to solutions of sundry other problems. While we rest satisfied with unanalyzed causes, we may be sure both that we do not rightly comprehend the production of the particular effects ascribed to them, and that we overlook other effects which would be revealed to us by contemplation of the causes as analyzed. Especially must this be so where the causation is complex. Hence we may infer that the phenomena presented by the development of species, are not likely to be truly conceived unless we keep in view the concrete agencies work. Let us look closely at the facts to be dealt wi

The growth of a thing is effected by the joint of certain forces on certain materials; and when it dwindles, there is either a lack of some materials, or the forces co-operate in a way different from that which produces growth. If a structure has varied, the implication is that the processes which built it up were made unlike the parallel processes in other cases, by the greater or less amount of some one or more of the matters or actions concerned. Where there is unusual fertility, the play of vital activities is thereby shown to have deviated from the ordinary play of vital activities; and conversely, if there is infertility. If the germs, or ova, or seed, or offspring partially developed, survive more or survive less, it is either because their molar or molecular structures are unlike the average ones, or because they are affected in unlike ways by surrounding agencies. When life is prolonged, the fact implies that the combination of actions, visible and invisible, constituting life, retains its equilibrium longer than usual in presence of environing forces which tend to destroy its equilibrium. That is to say, growth, variation, survival, death, if they are to be reduced to the forms in which physical science can recognize them, must be expressed as effects of agencies definitely conceived—mechanical forces, light, heat, chemical affinity, &c.

This general conclusion brings with it the thought that the phrases employed in discussing organic evolution, though convenient and indeed needful, are liable to mislead us by veiling the actual agencies. That which really goes on in every organism is the working together of component parts in ways conducing to the continuance of their combined actions, in presence of things and actions outside; some of which tend to subserve, and others to destroy, the combination. The matters and forces in these two groups, are the sole causes properly so called. The words "natural selection," do not express a cause in the physical sense. They express a mode of co-operation among causes—or rather, to speak strictly, they express an effect of this mode of co-operation. The idea they convey seems perfectly intelligible. Natural selection having been compared with artificial selection, and the analogy pointed out, there apparently remains no indefiniteness: the inconvenience being, however, that the definiteness is of a wrong kind. The tacitly implied Nature which selects, is not an embodied agency analogous to the man who selects artificially; and the selection is not the picking out of an individual fixed on, but the overthrowing of many individuals by agencies which one successfully resists, and hence continues to live and multiply. Mr. Darwin was conscious of these misleading implications. In the introduction to his *Animals and Plants under Domestication* (p. 6) he says:—

> "For brevity sake I sometimes speak of natural selection as an intelligent power; . . . I have, also, often personified the word Nature; for I have found it difficult to avoid this ambiguity; but I mean by nature only the aggregate action and product of many natural laws,—and by laws only the ascertained sequence of events."

But while he thus clearly saw, and distinctly asserted, that the factors of organic evolution are the concrete actions, inner and outer, to which every organism is subject, Mr. Darwin, by habitually using the convenient figure of speech, was, I think, prevented from recognizing so fully as he would otherwise have done, certain fundamental consequences of these actions.

Though it does not personalize the cause, and does not assimilate its mode of working to a human mode of working, kindred objections may be urged against the expression to which I was led when seeking to present the phenomena in literal terms rather than metaphorical terms—the survival of the fittest;[*] for in a vague way the first word, and in a clear way the second word, calls up an anthropocentric idea. The thought of survival inevitably suggests the human view of certain sets of phenomena, rather than that character which they have simply as groups of changes. If, asking what we really know of a plant, we exclude all the ideas associated with the words life and death, we find that the sole facts known to us are that there go on in the plant certain inter-dependent processes, in presence of certain aiding and hindering influences outside of it; and that in some cases a difference of structure or a favourable set of circumstances, allows these inter-dependent processes to go on for longer periods than in other cases. Again, in the working together of those many actions, internal and external, which determine the lives or deaths of organisms, we see nothing to which the words fitness and unfitness are applicable in the physical sense. If a key fits a lock, or a glove a hand, the relation of the things to one another is presentable to the perceptions. No approach to fitness of this kind is made by an organism which continues to live under certain conditions. Neither the organic structures themselves, nor their individual movements, nor those combined movements of certain among them which constitute conduct, are related in any analogous way to the things and actions in the environment. Evidently the word fittest, as thus used, is a figure of speech; suggesting the fact that amid surrounding actions, an organism characterized by the word has either a greater ability than others of its kind to maintain the equilibrium of its vital activities, or else has so much greater a power of multiplication that though not longer lived than they, it continues to live in posterity more persistently. And indeed, as we here see, the word fittest has to cover cases in which there may be less ability than usual to survive individually, but in which the defect is more than made good by higher degrees of fertility.

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[*] Though Mr. Darwin approved of this expression and occasionally employed it, he did not adopt it for general use; contending, very truly, that the expression Natural Selection is in some [illegible] convenient. See *Animals and Plants under Domestication* [illegible] and *Origin of Species* (sixth edition) p. 49.

I have elaborated this criticism with the intention of emphasizing the need for studying the changes which have gone on, and are ever going on, in organic bodies, from an exclusively physical point of view. On contemplating the facts from this point of view, we become aware that, besides those special effects of the co-operating forces which eventuate in the longer survival of one individual than of others, and in the consequent increase through generations, of some trait which furthered its survival, many other effects are being wrought on each and all of the individuals. Bodies of every class and quality, inorganic as well as organic, are from instant to instant subject to the influences in their environments; are from instant to instant being changed by these in ways that are mostly inconspicuous; and are in course of time changed by them in conspicuous ways. Living things in common with dead things, are, I say, being thus perpetually acted upon and modified; and the changes hence resulting, constitute an all-important part of those undergone in the course of organic evolution. I do not mean to imply that changes of this class pass entirely unrecognized; for, as we shall see, Mr. Darwin takes cognizance of certain secondary and special ones. But the effects which are not taken into account, are those primary and universal effects which give certain fundamental characters to all organisms. Contemplation of an analogy will best prepare the way for appreciation of them, and of the relation they bear to those which at present monopolize attention.

An observant rambler along shores, will, here and there, note places where the sea has deposited things more or less similar, and separated them from dissimilar things—will see shingle parted from sand; larger stones sorted from smaller stones; and will occasionally discover deposits of shells more or less worn by being rolled about. Sometimes the pebbles or boulders composing the shingle at one end of a bay, he will find much larger than those at the other: intermediate sizes, having small average differences, occupying the space between the extremes. An example occurs, if I remember rightly, some mile or two to the west of Tenby; but the most remarkable and well-known example is that afforded by the Chesil bank. Here, along a shore some sixteen miles long, there is a gradual increase in the sizes of the stones; which, being at one end but mere pebbles, are at the other end immense boulders. In this case, then, the breakers and the undertow have effected a selection—have at each place left behind those stones which were too large to be readily moved, while taking away others small enough to be moved easily. But now, if we contemplate exclusively this selective action of the sea, we overlook certain important effects which the sea simultaneously works. While the stones have been differently acted upon in so far that some have been left here and some carried there; they have been similarly acted upon in two allied, but distinguishable, ways. By perpetually rolling them about and knocking them one against another, the waves have so broken off their most prominent parts as to produce in all of them more or less rounded forms; and then, further, the mutual friction of the stones simultaneously caused, has smoothed their surfaces. That is to say in general terms, the actions of environing agencies, so far as they have operated indiscriminately, have produced in the stones a certain unity of character; at the same time that they have, by their differential effects, separated them: the larger ones having withstood certain violent actions which the smaller ones could not withstand.

Similarly with other assemblages of objects which are alike in their primary traits but unlike in their secondary traits. When simultaneously exposed to the same set of actions, some of these actions, rising to a certain intensity, may be expected to work on particular members of the assemblage changes which they cannot work in those which are markedly unlike; while others of the actions will work in all of them similar changes, because of the uniform relations between these actions and certain attributes common to all members of the assemblage. Hence it is inferable that on living organisms, which form an assemblage of this kind, and are unceasingly exposed in common to the agencies composing their inorganic environments, there must be wrought two such sets of effects. There will result a universal likeness among them consequent on the likeness of their respective relations to the matters and forces around; and there will result, in some cases, the differences due to the differential effects of these matters and forces, and in other cases, the changes which, being life-sustaining or life-destroying, eventuate in certain natural selections.

I have, above, made a passing reference to the fact that Mr. Darwin did not fail to take account of some among these effects directly produced on organisms by surrounding inorganic agencies. Here are extracts from the sixth edition of the *Origin of Species* showing this.

> "It is very difficult to decide how far changed conditions, such as of climate, food, &c., have acted in a definite manner. There is reason to believe that in the course of time the effects have been greater than can be proved by clear evidence. . . . Mr. Gould believes that birds of the same species are more brightly coloured under a clear atmosphere, than when living near the coast or on islands; and Wollaston is convinced that residence near the sea affects the colours of insects. Moquin-Tandon gives a list of plants which, when growing near the sea-shore, have their leaves in some degree fleshy, though not elsewhere fleshy" (pp. 106-7). "Some observers are convinced that a damp climate affects the growth of the hair, and that with the hair the horns are correlated" (p. 159).

In his subsequent work, *Animals and Plants under Domestication*, Mr. Darwin still more clearly recognizes these causes of change in organization. A chapter is devoted to the subject. After premising that "the direct action of the conditions of life, whether leading to definite or indefinite results, is a totally distinct consideration from the effects of natural selection;" he goes on to say that changed conditions of life "have acted so definitely and powerfull [illegible] tion of our domesticated productions, that they have sufficed to form new sub-varieties or races, without the aid of selection by man or of natural selection." Of his examples here are two.

"I have given in detail in the ninth chapter the most remarkable case known to me, namely, that in Germany several varieties of maize brought from the hotter parts of America were transformed in the course of only two or three generations." (Vol. ii, p. 277.) [And in this ninth chapter concerning these and other such instances he says "some of the foregoing differences would certainly be considered of specific value with plants in a state of nature." (Vol. i, p. 321.)] "Mr. Meehan, in a remarkable paper, compares twenty-nine kinds of American trees, belonging to various orders, with their nearest European allies, all grown in close proximity in the same garden and under as nearly as possible the same conditions." And then enumerating six traits in which the American forms all of them differ in like ways from their allied European forms, Mr. Darwin thinks there is no choice but to conclude that these "have been definitely caused by the long-continued action of the different climate of the two continents on the trees." (Vol. ii, pp. 281-2.)

But the fact we have to note is that while Mr. Darwin thus took account of special effects due to special amounts and combinations of agencies in the environment, he did not take account of the far more important effects due to the general and constant operation of these agencies.* If a difference between the quantities of a force which acts on two organisms, otherwise alike and otherwise similarly conditioned, produces some difference between them; then, by implication, this force produces in both of them effects which they show in common. The inequality between two things cannot have a value unless the things themselves have values. Similarly if, in two cases, some unlikeness of proportion among the surrounding inorganic agencies to which two plants or two animals are exposed, is followed by some unlikeness in the changes wrought on them; then it follows that these several agencies taken separately, work changes in both of them. Hence we must infer that organisms have certain structural characters in common, which are consequent on the action of the medium in which they exist: using the word medium in a comprehensive sense, as including all physical forces falling upon them as well as matters bathing them. And we may conclude that from the primary characters thus produced there must result secondary characters.

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* It is true that while not deliberately admitted by Mr. Darwin, these effects are not denied by him. In his *Animals and Plants under Domestication* (vol. ii, 281), he refers to certain chapters in the *Principles of Biology*, in which I have discussed this general inter-action of the medium and the organism, and ascribed certain most general traits to it. But though, by his expressions, he implies a sympathetic attention to the argument, he does not in such way adopt the conclusion as to assign to this factor any share in the genesis of organic structures—much less that large share which I believe it has had. I did not myself at that time, nor indeed until quite recently, see how extensive and profound have been the influences on organization which, as we shall presently see, are traceable to the early results of this fundamental relation between organism and medium. I may add that it is in an essay on "Transcendental Physiology," first published in 1857, that the line of thought here followed out in its wider bearings, was first entered upon.

Before going on to observe those general traits of organisms due to the general action of the inorganic environment upon them, I feel tempted to enlarge on the effects produced by each of the several matters and forces constituting the environment. I should like to do this not only to give a clear preliminary conception of the ways in which all organisms are affected by these universally-present agents, but also to show that, in the first place, these agents modify inorganic bodies as well as organic bodies, and that, in the second place, the organic are far more modifiable by them than the inorganic. But to avoid undue suspension of the argument, I content myself with saying that when the respective effects of gravitation, heat, light, &c., are studied, as well as the respective effects, physical and chemical, of the matters forming the media, water and air, it will be found that while more or less operative on all bodies, each modifies organic bodies to an extent immensely greater than the extent to which it modifies inorganic bodies.

Here, not discriminating among the special effects which these various forces and matters in the environment produce on both classes of bodies, let us consider their combined effects, and ask—What is the most general trait of such effects?

Obviously the most general trait is the greater amount of change wrought on the outer surface than on the inner mass. In so far as the matters of which the medium is composed come into play, the unavoidable implication is that they act more on the parts directly exposed to them than on the parts sheltered from them. And in so far as the forces pervading the medium come into play, it is manifest that, excluding gravity, which affects outer and inner parts indiscriminately, the outer parts have to bear larger shares of their actions. If it is a question of heat, then the exterior must lose it or gain it faster than the interior; and in a medium which is now warmer and now colder, the two must habitually differ in temperature to some extent—at least where the size is considerable. If it is a question of light, then in all but absolutely transparent masses, the outer parts must undergo more of any change producible by it than the inner parts—supposing other things equal; by which I mean, supposing the case is not complicated by any such convexities of the outer surface as produce internal concentrations of rays. Hence then, speaking generally, the necessity is that the primary and almost universal effect of the converse between the body and its medium, is to differentiate its outside from its inside. I say almost universal, because where the body is both mechanically and chemically stable, like, for instance, a quartz crystal, the medium may fail to work either inner or outer change.

Of illustrations among inorganic bodies, a convenient one is supplied by an old cannon-ball that has been long lying exposed. A coating of rust, formed of flakes within flakes, incloses it; and this thickens year by year, until, perhaps, it reaches a stage at which its exterior loses as much by rain and wind as its interior gains by further oxidation of the iron. Most mineral masses—pebbles, boulders, rocks—if they show any effect of the environment at all, show it only by that disintegration of surface which follows the freezing of absorbed water: an effect which, though mechanical rather than chemical, equally illustrates the general truth. Occasionally a "rocking-stone" is thus produced. There are formed successive layers relatively friable in texture, each of which, thickest at the most exposed parts, and being presently lost by weathering, leaves the contained mass in a shape more rounded than before; until, resting on its convex under-surface, it is easily moved. But of all instances perhaps the most remarkable is one to be seen on the west bank of the Nile at Philæ, where a ridge of granite 100 feet high, has had its outer parts reduced in course of time to a collection of boulder-shaped masses, varying from say a yard in diameter to six or eight feet, each one of which shows in progress an exfoliation of successively-formed shells of decomposed granite: most of the masses having portions of such shells partially detached.

If, now, inorganic masses, relatively so stable in composition, thus have their outer parts differentiated from their inner parts, what must we say of organic masses, characterized by such extreme chemical instability?—instability so great that their essential material is named protein, to indicate the readiness with which it passes from one isomeric form to another. Clearly the necessary inference is that this effect of the medium must be wrought inevitably and promptly, wherever the relation of outer and inner has become settled: a qualification for which the need will be seen hereafter.

Beginning with the earliest and most minute kinds of living things, we necessarily encounter difficulties in getting direct evidence; since, of the countless species now existing, all have been subject during millions upon millions of years to the evolutionary process, and have had their primary traits complicated and obscured by those endless secondary traits which the natural selection of favourable variations has produced. Among protophytes it needs but to think of the multitudinous varieties of diatoms and desmids, with their elaborately-constructed coverings; or of the definite methods of growth and multiplication among such simple *Algæ* as the *Conjugatæ*; to see that most of their distinctive characters are due to inherited constitutions, which have been slowly moulded by survival of the fittest to this or that mode of life. To disentangle such parts of their developmental changes as are due to the action of the medium, is therefore hardly possible. We can hope only to get a general conception of it by contemplating the totality of the facts.

The first cardinal fact is that all protophytes are cellular—all show us this contrast between outside and inside. Supposing the multitudinous specialities of the envelope in different orders and genera of protophytes to be set against one another, and mutually cancelled, there remains as a trait common to them—an envelope unlike that which it envelopes. The second cardinal fact is that this simple trait is the earliest trait displayed in germs, or spores, or other parts from which new individuals are to arise; and that, consequently, this trait must be regarded as having been primordial. For it is an established truth of organic evolution that embryos show us, in general ways, the forms of remote ancestors; and that the first changes undergone, indicate, more or less clearly, the first changes which took place in the series of forms through which the existing form has been reached. Describing, in successive groups of plants, the early transformations of these primitive units, Sachs* says of the lowest *Algæ* that "the conjugated protoplasmic body clothes itself with a cell-wall" (p. 10); that in "the spores of Mosses and Vascular Cryptogams" and in "the pollen of Phanerogams" . . . "the protoplasmic body of the mother-cell breaks up into four lumps, which quickly round themselves off and contract, and become enveloped by a cell-membrane only after complete separation" (p. 13); that in the *Equisetaceæ* "the young spores, when first separated, are still naked, but they soon become surrounded by a cell-membrane" (p. 14); and that in higher plants, as in the pollen of many Dicotyledons, "the contracting daughter-cells secrete cellulose even during their separation" (p. 14). Here, then, in whatever way we interpret it, the fact is that there quickly arises an outer layer different from the contained matter. But the most significant evidence is furnished by "the masses of protoplasm that escape into water from the injured sacs of *Vaucheria*, which often instantly become rounded into globular bodies," and of which the "hyaline protoplasm envelopes the whole as a skin" (p. 41) which "is denser than the inner and more watery substance" (p. 42). As in this case the protoplasm is but a fragment, and as it is removed from the influence of the parent-cell, this differentiating process can scarcely be regarded as anything more than the effect of physico-chemical actions: a conclusion which is supported by the statement of Sachs that "not only every vacuole in a solid protoplasmic body, but also every thread of protoplasm which penetrates the sap-cavity, and finally the inner side of the protoplasm-sac which encloses the sap-cavity, is also bounded by a skin" (p. 42). If then "every portion of a protoplasmic body immediately surrounds itself, when it becomes isolated, with such a skin," which is shown in all cases to arise at the surface of contact with sap or water, this primary differentiation of outer from inner must be ascribed to the direct action of the medium. Whether the coating thus initiated is secreted by the protoplasm, or whether, as seems more likely, it results from transformation of it, matters not to the argument. Either way the action of the medium causes its formation; and either way the many varied and complex differentiations which developed cell-walls display, must be considered as originating from those variations of this physically-generated covering which natural selection has taken advantage of.

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* *Text-Book of Botany, &c.* by Julius Sachs. Translated by A. W. Bennett and W. T. T. Dyer.

The contained protoplasm of a vegetal cell, which has self-mobility and when liberated sometimes performs amœba-like motions for a time, may be regarded as an imprisoned amœba; and when we pass from it to a free amœba, which is one of the simplest types of first animals, or *Protozoa,* we naturally meet with kindred phenomena. The general trait which here concerns us, is that while its plastic or semi-fluid sarcode goes on protruding, in irregular ways, now this and now that part of its periphery, and again withdrawing into its interior first one and then another of these temporary processes, perhaps with some small portion of food attached, there is but an indistinct differentiation of outer from inner (a fact shown by the frequent coalescence of the pseudopodia in Rhizopods); but that when it eventually becomes quiescent, the surface becomes differentiated from the contents: the passing into an encysted state, doubtless in large measure due to inherited proclivity, being furthered, and having probably been once initiated, by the action of the medium. The connexion between constancy of relative position among the parts of the sarcode, and the rise of a contrast between superficial and central parts, is perhaps best shown in the minutest and simplest *Infusoria,* the *Monadinæ.* The genus *Monas* is described by Kent as "plastic and unstable in form, possessing no distinct cuticular investment; . . . the food-substances incepted at all parts of the periphery";* and the genus *Scytomonas* he says "differs from *Monas* only in its persistent shape and accompanying greater rigidity of the peripheral or ectoplasmic layer."* Describing generally such low forms, some of which are said to have neither nucleus nor vacuole, he remarks that in types somewhat higher "the outer or peripheral border of the protoplasmic mass, while not assuming the character of a distinct cell-wall or so-called cuticle, presents, as compared with the inner substance of that mass, a slightly more solid type of composition."† And it is added that these forms having so slightly differentiated an exterior, "while usually exhibiting a more or less characteristic normal outline, can revert at will to a pseud-amœboid and repent state."‡ Here, then, we have several indications of the truth that the permanent externality of a certain part of the substance, is followed by transformation of it into a coating unlike the substance it contains. Indefinite and structureless in the simplest of these forms, as instance again the *Gregarina*,§ the limiting membrane becomes, in higher *Infusoria*, definite and often complex: showing that the selection of favourable variations has had largely to do with its formation. In such types as the *Foraminifera*, which, almost structureless internally though they are, secrete calcareous shells, it is clear that the nature of this outer layer is determined by inherited constitution. But recognition of this consists with the belief that the action of the medium initiated the outer layer, specialized though it now is; and that even still, contact with the medium excites secretion of it.

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* *A Manual of the Infusoria,* by W. Saville Kent. Vol. i, p. 232.

A remarkable analogy remains to be named. When we study the action of the medium in an inorganic mass, we are led to see that between the outer changed layer and the inner unchanged mass, comes a surface where active change is going on. Here we have to note that, alike in the plant-cell and in the animal-cell, there is a similar relation of parts. Immediately inside the envelope comes the primordial utricle in the one case, and in the other case the layer of active sarcode. In either case the living protoplasm, placed in the position of a lining to the cuticle of the cell, is shielded from the direct action of the medium, and yet is not beyond the reach of its influences.

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* *Ib.* Vol. i, p. 241. † Kent, Vol. i, p. 56. ‡ *Ib.* Vol. i, p. 57.

§ *The Elements of Comparative Anatomy*, by T. H. Huxley, pp. 7-9.

Limited, as thus far drawn, to a certain common trait of those minute organisms which are mostly below the reach of unaided vision, the foregoing conclusion appears trivial enough. But it ceases to appear trivial on passing into a wider field, and observing the implications, direct and indirect, as they concern plants and animals of sensible sizes.

Popular expositions of science have so far familiarized many readers with a certain fundamental trait of living things around, that they have ceased to perceive how marvellous a trait it is, and, until interpreted by the Theory of Evolution, how utterly mysterious. In past times, the conception of an ordinary plant or animal which prevailed, not throughout the world at large only but among the most instructed, was that it is a single continuous entity. One of these livings things was unhesitatingly regarded as being in all respects a unit. Parts it might have, various in their sizes, forms, and compositions; but these were components of a whole which had been from the beginning in its original nature a whole. Even to naturalists fifty years ago, the assertion that a cabbage or a cow, though in one sense a whole, is in another sense a vast society of minute individuals, severally living in greater or less degrees, and some of them maintaining their independent lives unrestrained, would have seemed an absurdity. But this truth which, like so many of the truths established by science, is contrary to that common sense in which most people have so much confidence, has been gradually growing clear since the days when Leeuwenhoeck and his contemporaries began to examine through lenses the minute structures of common plants and animals. Each improvement in the microscope, while it has widened our knowledge of those minute forms of life described above, has revealed further evidence of the fact that all the larger forms of life consist of units severally allied in their fundamental traits to these minute forms of life. Though, as formulated by Schwann and Schleiden, the cell-doctrine has undergone qualifications of statement; yet the qualifications have not been such as to militate against the general proposition that organisms visible to the naked eye, are severally compounded of invisible organisms—using that word in its most comprehensive sense. And then, when the development of any animal is traced, it is found that having been primarily a nucleated cell, and having afterwards become by spontaneous fission a cluster of nucleated cells, it goes on through successive stages to form out of such cells, ever multiplying and modifying in various ways, the several tissues and organs composing the adult.

On the hypothesis of evolution this universal trait has to be accepted not as a fact that is strange but unmeaning. It has to be accepted as evidence that all the visible forms of life have arisen by union of the invisible forms; which, instead of flying apart when they divided, remained together. Various intermediate stages are known. Among plants, those of the *Volvox* type show us the component protophytes so feebly combined that they severally carry on their lives with no appreciable subordination to the life of the group. And among animals, a parallel relation between the lives of the units and the life of the group is shown us in *Uroglena* and *Syncrypta*. From these first stages upwards, may be traced through successively higher types, an increasing subordination of the units to the aggregate; though still a subordination leaving to them conspicuous amounts of individual activity. Joining which facts with the phenomena presented by the cell-multiplication and aggregation of every unfolding germ, naturalists are now accepting the conclusion that by this process of composition from *Protozoa,* were formed all classes of the *Metazoa**—(as animals formed by this compounding are now called); and that in a similar way from *Protophyta,* were formed all classes of what I suppose will be called *Metaphyta,* though the word does not yet seem to have become current.

And now what is the general meaning of these truths, taken in connexion with the conclusion reached in the last section. It is that this universal trait of the *Metazoa* and *Metaphyta,* must be ascribed to the primitive action and re-action between the organism and its medium. The operation of those forces which produced the primary differentiation of outer from inner in early minute masses of protoplasm, pre-determined this universal cell-structure of all embryos, plant and animal, and the consequent cell-composition of adult forms arising from them. How unavoidable is this implication, will be seen on carrying further an illustration already used—that of the shingle-covered shore, the pebbles on which, while being in some cases selected, have been in all cases rounded and smoothed. Suppose a bed of such shingle to be, as we often see it, solidified, along with interfused material, into a conglomerate. What in such case must be considered as the chief trait of such conglomerate; or rather—what must we regard as the chief cause of its distinctive characters? Evidently the action of the sea. Without the breakers, no pebbles; without the pebbles, no conglomerate. Similarly then, in the absence of that action of the medium by which was effected the differentiation of outer from inner in those microscopic portions of protoplasm constituting the earliest and simplest animals and plants, there could not have existed this cardinal trait of composition which all the higher animals and plants show us.

So that, active as has been the part played by natural selection, alike in modifying and moulding the original

* *A Treatise on Comparative Embryology,* by F. M. Balfour, Vol. ii, chap. xiii.

units—largely as survival of the fittest has been instrumental in furthering and controlling the combination of these units into visible organisms, and eventually into large ones; yet we must ascribe to the direct effect of the medium on the first forms of life, that character of which this everywhere-operative factor has taken advantage.

Let us turn now to another and more obvious attribute of higher organisms, for which also there is this same general cause. Let us observe how, on a higher platform, there recurs this differentiation of outer from inner—how this primary trait in the living units with which life commences, re-appears as a primary trait in those aggregates of such units which constitute visible organisms.

In its simplest and most unmistakable form, we see this in the early changes of an unfolding ovum of primitive type. The original fertilized single cell, having by spontaneous fission multiplied into a cluster of such cells, there begins to show itself a contrast between periphery and centre; and presently there is formed a sphere consisting of a superficial layer unlike its contents. The first change, then, is the rise of a difference between that outer part which holds direct converse with the surrounding medium, and that inclosed part which does not. This primary differentiation in these compound embryos of higher animals, parallels the primary differentiation undergone by the simplest living things.

Leaving, for the present, succeeding changes of the compound embryo, the significance of which we shall have to consider by-and-by, let us pass now to the adult forms of visible plants and animals. In them we find cardinal traits which, after what we have seen above, will further impress us with the importance of the effects wrought on the organism by its medium.

From the thallus of a sea-weed up to the leaf of a highly developed phænogam, we find, at all stages, a contrast between the inner and outer parts of these flattened masses of tissue. In the higher *Algæ* "the outermost layers consist of smaller and firmer cells, while the inner cells are often very large, and sometimes extremely long;"* and in the leaves of trees the epidermal layer, besides differing in the sizes and shapes of its component cells from the parenchyma forming the inner substance of the leaf, is itself differentiated by having a continuous cuticle, and by having the outer walls of its cells unlike the inner walls.† Especially significant is the structure of such intermediate types as the Liverworts. Beyond the differentiation of the covering cells from the contained cells, and the contrast between upper surface and under surface, the frond of *Marchantia polymorpha* clearly shows us the direct effect of incident forces; and shows us, too, how it is involved with the effect of inherited proclivities. The frond grows from a flat disc-shaped gemma, the two sides of which are alike Either side may fall uppermost; and then of the developing shoot, the side exposed to the light "is under al circumstances the upper side which forms stomata, the dark side becomes the under side which produces root-hairs and leafy processes."‡ So that while we have undeniable proof that the contrasted influences of the medium on the two sides, initiate the differentiation, we have also proof that the completion of it is determined by the transmitted structure of the type; since it is impossible to ascribe the development of stomata to the direct action of air and light. On turning from foliar expansions, to stems and roots, facts of like meaning meet us. Speaking generally of epidermal tissue and inner tissue, Sachs remarks that "the contrast of the two is the plainer the more the part of the plant concerned is exposed to air and light."§ Elsewhere, in correspondence with this, it is said that in roots the cells of the epidermis though distinguished by bearing hairs, "are otherwise similar to those of the fundamental tissue" which they clothe,* while the cuticular covering is relatively thin; whereas in stems the epidermis (often further differentiated) is composed of layers of cells which are smaller and thicker-walled: a stronger contrast of structure corresponding to a stronger contrast of conditions. By way of meeting the suggestion that these respective differences are wholly due to the natural selection of favourable variations, it will suffice if I draw attention to the unlikeness between imbedded roots and exposed roots. While in darkness, and surrounded by moist earth, the outermost protective coats, even of large roots, are comparatively thin; but when the accidents of growth entail permanent exposure to light and air, roots acquire coverings allied in character to the coverings of branches. That the action of the medium causes these and converse changes, cannot be doubted when we find, on the one hand, that "roots can become directly transformed into leaf-bearing shoots," and, on the other hand, that in some plants certain "apparent roots are only underground shoots," and that nevertheless "they are similar to true roots in function and in the formation of tissue, but have no root-cap, and, when they come to the light above ground, continue to grow in the manner of ordinary leaf-shoots."† If, then, in highly developed plants inheriting pronounced structures, this differentiating influence of the medium is so marked, it must have been all-important at the outset while types were undetermined.

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* Sachs, p. 210. † *Ibid.* pp. 83-4. ‡ *Ibid.* p. 185. § *Ibid.* 80.

As with plants so with animals, we find good reason for inferring that while the specialities of the tegumentary parts must be ascribed to the natural selection of favourable variations, their most general traits are due to the direct action of surrounding agencies. Here we come upon the border of those changes which are ascribable to use and disuse. But from this class of changes we may fitly exclude those in which the parts concerned are wholly or

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* Sachs, p. 83.

† *Ibid.* p. 147.

mainly passive. A corn and a blister will conveniently serve to illustrate the way in which certain outer actions initiate in the superficial tissues, effects of very marked kinds, which are related neither to the needs of the organism nor to its normal structure. They are neither adaptive changes nor changes towards completion of the type. After noting them we may pass to allied, but still more instructive, changes. Continuous pressure on any portion of the surface causes absorption, while intermittent pressure causes growth: the one impeding circulation and the passage of plasma from the capillaries into the tissues, and the other aiding both. There are yet further mechanically-produced effects. That the general character of the ribbed skin on the under surfaces of the feet and insides of the hands is directly due to friction and intermittent pressure, we have the proofs:—first, that the tracts most exposed to rough usage are the most ribbed; second, that the insides of hands subject to unusual amounts of rough usage, as those of sailors, are strongly ribbed all over; and third, that in hands which are very little used, the parts commonly ribbed become quite smooth. These several kinds of evidence, however, full of meaning as they are, I give simply to prepare the way for evidence of a much more conclusive kind.

Where a wide ulcer has eaten away the deep-seated layer out of which the epidermis grows, or where this layer has been destroyed by an extensive burn, the process of healing is very significant. From the subjacent tissues, which in the normal order have no concern with outward growth, there is produced a new skin, or rather a pro-skin; for this substituted outward-growing layer contains no hair-follicles or other specialities of the original one. Nevertheless, it is like the original one in so far that it is a continually renewed protective covering. Doubtless it may be contended that this make-shift skin results from the inherited proclivity of the type—the tendency to complete afresh the structure of the species when injured. We cannot, however, ignore the immediate influence of the medium, on recalling the facts above named, or on remembering the further fact that an inflamed surface of skin, when not sheltered from the air, will throw out a film of coagulable lymph. But that the direct action of the medium is a chief factor we are clearly shown by another case. Accident or disease occasionally causes permanent eversion, or protrusion, of mucous membrane. After a period of irritability, great at first but decreasing as the change advances, this membrane assumes the general character of ordinary skin. Nor is this all: its microscopic structure changes. Where it is a mucous membrane of the kind covered by cylinder-epithelium, the cylinders gradually shorten, becoming finally flat, and there results a squamous epithelium: there is a near approach in minute composition to epidermis. Here a tendency towards completion of the type cannot be alleged; for there is, contrariwise, divergence from the type. The effect of the medium is so great that, in a short time, it overcomes the inherited proclivity and produces a structure of opposite kind to the normal one.

With but little break we come here upon a significant analogy, parallel to an analogy already described. As was pointed out, an inorganic body that is modifiable by its medium, acquires, after a time, an outer coat which has already undergone such change as surrounding agencies can effect; has a contained mass which is as yet unchanged, because unreached; and has a surface between the two where change is going on—a region of activity. And we saw that alike in the vegetal cell and the animal cell there exist analogous distributions: of course with the difference that the innermost part is not inert. Now we have to note that in those aggregates of cells constituting the *Metaphyta* and *Metazoa*, analogous distributions also exist. In plants they are of course not to be looked for in leaves and other deciduous portions, but only in portions of long duration—

stems and branches. Naturally, too, we need not expect them in plants having modes of growth which early produce an outer practically dead part, that effectually shields the inner actively living part of the stem from the influence of the medium—long-lived acrogens such as tree-ferns and long-lived endogens such as palms. But in the highest plants, exogens, which have the actively living part of their stems within reach of environing agencies, we find this part,—the cambium layer,—is one from which there is a growth inwards forming wood, and a growth outwards forming bark: there is an increasingly thick covering (where it does not scale off) of tissue changed by the medium, and inside this a film of highest vitality. In so far as concerns the present argument, it is the same with the *Metazoa*, or at least all of them which have developed organizations. The outer skin grows up from a limiting plane, or layer, a little distance below the surface—a place of predominant vital activity. Here perpetually arise new cells, which, as they develop, are thrust outwards and form the epidermis: flattening and drying up as they approach the surface, whence, having for a time served to shield the parts below, they finally scale off and leave younger ones to take their places. This still undifferentiated tissue forming the base of the epidermis, and existing also as a source of renewal in internal organs, is the essentially living substance; and facts above given imply that it was the action of the medium on this essentially living substance, which, during early stages in the organization of the *Metazoa*, initiated that protective envelope which presently became an inherited structure—a structure which, though now mainly inherited, still continues to be modifiable by its initiator.

Fully to perceive the way in which these evidences compel us to recognize the influence of the medium as a primordial factor, we need but conceive them as interpreted without it. Suppose, for instance, we say that the structure of the epidermis is wholly determined by the natural selection of favourable variations; what must be the position taken in presence of the fact above named, that when mucous membrane is exposed to the air its cell-structure changes into the cell-structure of skin? The position taken must be this :—Though mucous membrane in a highly-evolved individual organism, thus shows the powerful effect of the medium on its surface; yet we must not suppose that the medium had the effect of producing such a cell-structure on the surfaces of primitive forms, undifferentiated though they were; or, if we suppose that such an effect was produced on them, we must not suppose that it was inheritable. Contrariwise, we must suppose that such effect of the medium either was not wrought at all, or that it was evanescent: though repeated through millions upon millions of generations it left no traces. And we must conclude that this skin-structure arose only in consequence of spontaneous variations not physically initiated (though like those physically initiated) which natural selection laid hold of and increased. Does any one think this a tenable position?

And now we approach the last and chief series of morphological phenomena which must be ascribed to the direct action of environing matters and forces. These are presented to us when we study the early stages in the development of the embryos of the *Metazoa* in general.

We will set out with the fact already noted in passing, that after repeated spontaneous fissions have changed the original fertilized germ-cell into that cluster of cells which forms a gemmule or a primitive ovum, the first contrast which arises is between the peripheral parts and the central parts. Where, as with lower creatures which do not lay up large stores of nutriment with the germs of their offspring, the inner mass is inconsiderable, the outer layer of cells, which are presently made quite small by repeated subdivision forms a membrane extending over the whole surface—th blastoderm. The next stage of development, which end in this covering layer becoming double, is reached in tw ways—by invagination and by delamination; but which is th original way and which the abridged way, is not quite certain. Of invagination, multitudinously exemplified in th lowest types, Mr. Balfour says:—"On purely *à priori* ground there is in my opinion more to be said for invaginatio than for any other view";* and, for present purposes, i will suffice if we limit ourselves to this: making its natur clear to the general reader by a simple illustration.

Take a small india-rubber ball—not of the inflated kind nor of the solid kind, but of the kind about an inch or s in diameter with a small hole through which, under pressure the air escapes. Suppose that instead of consisting of india-rubber its wall consists of small cells made polyhedral i form by mutual pressure, and united together. This wil represent the blastoderm. Now with the finger, thrust i one side of the ball until it touches the other: so making cup. This action will stand for the process of invagination. Imagine that by continuance of it, the hemispherical cu becomes very much deepened and the opening narrowed, until the cup becomes a sac, of which the introverted wall is everywhere in contact with the outer wall. This will represent the two-layered "gastrula"—the simplest ancestral form of the *Metazoa:* a form which is permanently represented in some of the lowest types; for it needs but tentacles round the mouth of the sac, to produce a common hydra. Here the fact which it chiefly concerns us to remark, is that of these two layers the outer, called in embryological language the epiblast, continues to carry on direct converse with the forces and matters in the environment; while the inner, called the hypoblast, comes in contact with such only of these matters as are put into the food-cavity which it lines. We have further to note that in the embryos of *Metazoa* at all advanced in organization, there arises between these two layers a third—the mesoblast. The origin of this is seen in types where the developmental process is not obscured by the presence of a large food-yolk. While the above-described introversion is taking place, and before the inner surfaces of the resulting epiblast and hypoblast have come into contact, cells, or amœboid units equivalent to them, are budded off from one or both of these inner surfaces, or some part of one or other; and these form a layer which eventually lies between the other two—a layer which, as this mode of formation implies, never has any converse with the surrounding medium and its contents, or with the nutritive bodies taken in from it. The striking facts to which this description is a necessary introduction, may now be stated. From the outer layer, or epiblast, are developed the permanent epidermis and its out-growths, the nervous system, and the organs of sense. From the introverted layer, or hypoblast, are developed the alimentary canal and those parts of its appended organs, liver, pancreas, &c., which are concerned in delivering their secretions into the alimentary canal, as well as the linings of those ramifying tubes in the lungs which convey air to the places where gaseous exchange is effected. And from the mesoblast originate the bones, the muscles, the heart and blood-vessels, and the lymphatics, together with such parts of various internal organs as are most remotely concerned with the outer world. Minor qualifications being admitted, there remain the broad general facts, that out of that part of the external layer which remains permanently external, are developed all the structures which carry on intercourse with the medium and its contents, active and passive; out of the introverted part of this external layer, are developed the structures which carry on intercourse with the quasi-external substances that are taken into the interior—solid food, water, and air; while out of the mesoblast are developed structures which have never had, from first to last, any intercourse with the environment. Let us contemplate these general facts.

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* *A Treatise on Comparative Embryology.* By Francis M. Balfour, LL.D., F.R.S. Vol. ii, p. 343 (second edition).

Who would have imagined that the nervous system is a modified portion of the primitive epidermis? In the absence of proofs furnished by the concurrent testimony of embryologists during the last thirty or forty years, who would have believed that the brain arises from an infolded tract of the outer skin, which, sinking down beneath the surface, becomes imbedded in other tissues and eventually surrounded by a bony case? Yet the human nervous system in common with the nervous systems of lower animals is thus originated. In the words of Mr. Balfour, early embryological changes imply that—

> "the functions of the central nervous system, which were originally taken by the whole skin, became gradually concentrated in a special part of the skin which was step by step removed from the surface, and has finally become in the higher types a well-defined organ imbedded in the subdermal tissues. . . . The embryological evidence shows that the ganglion-cells of the central part of the nervous system are originally derived from the simple undifferentiated epithelial cells of the surface of the body."*

Less startling perhaps, though still startling enough, is the fact that the eye is evolved out of a portion of the skin; and that while the crystalline lens and its surroundings thus originate, the "percipient portions of the organs of special sense, especially of optic organs, are often formed from the same part of the primitive epidermis" which forms the central nervous system.† Similarly is it with the organs for smelling and hearing. These, too, begin as sacs formed by infoldings of the epidermis; and while their parts are developing they are joined from within by nervous structures which were themselves epidermic in origin. How are we to interpret these strange transformations? Observing, as we pass, how absurd from the point of view of the special-creationist, would appear such a filiation of structures, and such a round-about mode of embryonic development, we have here to remark that the process is not one to have been anticipated as a result of natural selection. After numbers of spontaneous variations had occurred, as the hypothesis implies, in useless ways, the variation which primarily initiated a nervous centre might reasonably have been expected to occur in some internal part where it would be fitly located. Its initiation in a dangerous place and subsequent migration to a safe place, would be incomprehensible. Not so if we bear in mind the cardinal truth above set forth, that the structures for holding converse with the medium and its contents, arise in that completely superficial part which is directly affected by the medium and its contents; and if we draw the inference that the external actions themselves initiate the structures. These once commenced, and furthered by natural selection where favourable to life, would form the first term of a series ending in developed sense organs and a developed nervous system.*

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* Balfour, l.c. Vol. ii, 400-1.

† Balfour, l.c. Vol. ii, p. 401.

Though it would enforce the argument, I must, for brevity's sake, pass over the analogous evolution of that introverted layer, or hypoblast, out of which the alimentary canal and attached organs arise. It will suffice to emphasize the fact that having been originally external, this layer continues in its developed form to have a quasi-externality, alike in its digesting part and in its respiratory part; since it continues to deal with matters alien to the organism. I must also refrain from dwelling at length on the fact already adverted to, that the intermediate derived layer, or mesoblast, which was at the outset completely internal, originates those structures which ever remain completely internal, and have no communication with the environment save through the structures developed from the other two: an antithesis which has great significance.

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* For a general delineation of the changes by which the development is effected, see Balfour, l.c. Vol. ii, pp. 401-4.

Here, instead of dwelling on these details, it will be better to draw attention to the most general aspect of the facts. Whatever may be the course of subsequent changes, the first change is the formation of a superficial layer or blastoderm; and by whatever series of transformations the adult structure is reached, it is from the blastoderm that all the organs forming the adult originate. Why this marvellous fact?

Meaning is given to it if we go back to the first stage in which *Protozoa,* having by repeated fissions formed a cluster, then arranged themselves into a hollow sphere, as do the protophytes forming a *Volvox.* Originally alike all over its surface, the hollow sphere of ciliated units thus formed, would, if not quite spherical, assume a constant attitude when moving through the water; and hence one part of the spheroid would more frequently than the rest come in contact with nutritive matters to be taken in. A division of labour resulting from such a variation being advantageous, and tending therefore to increase in descendants, would end in a differentiation like that shown in the gemmules of various low types of *Metazoa,* which, ovate in shape, are ciliated over one part of the surface only. There would arise a form in which the cilium-bearing units effected locomotion and aeration; while on the others, assuming an amœba-like character, devolved the function of absorbing food: a primordial specialization variously indicated by evidence.* Just noting that an ancestral origin of this kind is implied by the fact that in low types of *Metazoa* a hollow sphere of cells is the form first assumed by the unfolding embryo, I draw attention to the point here of chief interest; namely that the primary differentiation of this hollow sphere is in such case determined by a difference in the converse of its parts with the medium and its contents; and that the subsequent invagination arises by a continuance of this differential converse.

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* *See* Balfour, Vol. i, 149 and Vol. ii, 343-4.

Even neglecting this first stage and commencing with the next, in which a "gastrula" has been produced by the permanent introversion of one portion of the surface of the hollow sphere, it will suffice if we consider what must thereafter have happened. That which continued to be the outer surface was the part which from time to time touched quiescent masses and occasionally received the collisions consequent on its own motions or the motions of other things. It was the part to receive the sound-vibrations occasionally propagated through the water; the part to be affected more strongly than any other by those variations in the amounts of light caused by the passing of small bodies close to it; and the part which met those diffused molecules constituting odours. That is to say, from the beginning the surface was the part on which there fell the various influences pervading the environment, the part by which there was received those impressions from the environment serving for the guidance of actions, and the part which had to bear the mechanical re-actions consequent upon such actions. Necessarily, therefore, the surface was the part in which were initiated the various instrumentalities for carrying on intercourse with the environment. To suppose otherwise is to suppose that such instrumentalities arose internally where they could neither be operated on by surrounding agencies nor operate on them,—where the differentiating forces did not come into play, and the differentiated structures had nothing to do; and it is to suppose that meanwhile the parts directly exposed to the differentiating forces remained unchanged. Clearly, then, organization could not but begin on the surface; and having thus begun, its subsequent course could not but be determined by its superficial origin. And hence these remarkable facts showing us that individual evolution is accomplished by successive in-foldings and in-growings. Doubtless natural selection soon came into action, as, for example, in the removal of the rudimentary nervous centres from the surface; since an individual in which they were a little more deeply seated would be less likely to be incapacitated by injury of them. And so in multitudinous other ways. But nevertheless, as we here see, natural selection could operate only under subjection. It could do no more than take advantage of those structural changes which the medium and its contents initiated.

See, then, how large has been the part played by this primordial factor. Had it done no more than give to *Protozoa* and *Protophyta* that cell-form which characterizes them—had it done no more than entail the cellular composition which is so remarkable a trait of *Metazoa* and *Metaphyta*—had it done no more than cause the repetition in all visible animals and plants of that primary differentiation of outer from inner which it first wrought in animals and plants invisible to the naked eye; it would have done much towards giving to organisms of all kinds certain leading traits. But it has done more than this. By causing the first differentiations of those clusters of units out of which visible animals in general arose, it fixed the starting place for organization, and therefore determined the course of organization; and, doing this, gave indelible traits to embryonic transformations and to adult structures.

Though mainly carried on after the inductive method, the argument at the close of the foregoing section has passed into the deductive. Here let us follow for a space the deductive method pure and simple. Doubtless in biology *à priori* reasoning is dangerous; but there can be no danger in considering whether its results coincide with those reached by reasoning *à posteriori*.

Biologists in general agree that in the present state of the world, no such thing happens as the rise of a living creature out of non-living matter. They do not deny, however, that at a remote period in the past, when the temperature of the Earth's surface was much higher than at present, and other physical conditions were unlike those we know, inorganic matter, through successive complications, gave origin to organic matter. So many substances once supposed to belong exclusively to living bodies, have now been formed artificially, that men of science scarcely question the conclusion that there are conditions under which, by yet another step of composition, quaternary compounds of lower types pass into those of highest types. That there once took place gradual divergence of the organic from the inorganic, is, indeed, a necessary implication of the hypothesis of Evolution, taken as a whole; and if we accept it as a whole, we must put to ourselves the question—What were the early stages of progress which followed, after the most complex form of matter had arisen out of forms of matter a degree less complex?

At first, protoplasm could have had no proclivities to one or other arrangement of parts; unless, indeed, a purely mechanical proclivity towards a spherical form when suspended in a liquid. At the outset it must have been passive. In respect of its passivity, primitive organic matter must have been like inorganic matter. No such thing as spontaneous variation could have occurred in it; for variation implies some habitual course of change from which it is a divergence, and is therefore excluded where there is no habitual course of change. In the absence of that cyclical series of metamorphoses which even the simplest living thing now shows us, as a result of its inherited constitution, there could be no *point d'appui* for natural selection. How, then, did organic evolution begin?

If a primitive mass of organic matter was like a mass of inorganic matter in respect of its passivity, and differed only in respect of its greater changeableness; then we must infer that its first changes conformed to the same general law as do the changes of an inorganic mass. The instability of the homogeneous is a universal principle.

In all cases the homogeneous tends to pass into the heterogeneous, and the less heterogeneous into the more heterogeneous. In the primordial units of protoplasm, then, the step with which evolution commenced must have been the passage from a state of complete likeness throughout the mass to a state in which there existed some unlikeness. Further, the cause of this step in one of these portions of organic matter, as in any portion of inorganic matter, must have been the different exposure of its parts to incident forces. What incident forces? Those of its medium or environment. Which were the parts thus differently exposed? Necessarily the outside and the inside. Inevitably, then, alike in the organic aggregate and the inorganic aggregate (supposing it to have coherence enough to maintain constant relative positions among its parts), the first fall from homogeneity to heterogeneity must always have been the differentiation of the external surface from the internal contents. No matter whether the modification was physical or chemical, one of composition or of decomposition, it comes within the same generalization. The direct action of the medium was the primordial factor of organic evolution.

And now, finally, let us look at the factors in their *ensemble*, and consider the respective parts they play: observing, especially, the ways in which, at successive stages, they severally give place one to another in degree of importance.

Acting alone, the primordial factor must have initiated the primary differentiation in all units of protoplasm alike. I say alike, but I must forthwith qualify the word. For since surrounding influences, physical and chemical, could not be absolutely the same in all places, especially when the first rudiments of living things had spread over a considerable area, there necessarily arose small contrasts between the degrees and kinds of superficial differentiation effected. As soon as these became decided, natural selection came into play; for inevitably the unlikenesses produced among the units had effects on their lives: there was survival of some among the modified forms rather than others. Utterly in the dark though we are respecting the causes which set up that process of fission everywhere occurring among the minutest forms of life, we must infer that, when established, it furthered the spread of those which were most favourably differentiated by the medium. Though natural selection must have become increasingly active when once it had got a start; yet the differentiating action of the medium never ceased to be a co-operator in the development of these first animals and plants. Again taking the lead as there arose the composite forms of animals and plants, and again losing the lead with that advancing differentiation of these higher types which gave more scope to natural selection, it nevertheless continued, and must ever continue, to be a cause, both direct and indirect, of modifications in structure.

Along with that remarkable process which, beginning in minute forms with what is called conjugation, developed into sexual generation, there came into play causes of frequent and marked fortuitous variations. The mixtures of constitutional proclivities made more or less unlike by unlikenesses of physical conditions, inevitably led to occasional concurrences of forces producing deviations of structure. These were of course mostly suppressed, but sometimes increased, by survival of the fittest. When, along with the growing multiplication in forms of life, conflict and competition became continually more active, fortuitous variations of structure of no account in the converse with the medium, became of much account in the struggle with enemies and competitors; and natural selection of such variations became the predominant factor. Especially throughout the plant-world its action appears to ha been immensely the most important; and throughout that large part of the animal world characterized by relative inactivity, the survival of individuals that had varied in favourable ways, must all along have been the chief cause of the divergence of species and the occasional production of higher ones.

But gradually with that increase of activity which we see on ascending to successively higher grades of animals, and especially with that increased complexity of life which we also see, there came more and more into play as a factor, the inheritance of those modifications of structure caused by modifications of function. Eventually, among creatures of high organization, this factor became an important one; and I think there is reason to conclude that, in the case of the highest of creatures, civilized men, among whom the kinds of variation which affect survival are too multitudinous to permit easy selection of any one, and among whom survival of the fittest is greatly interfered with, it has become the chief factor: such aid as survival of the fittest gives, being usually limited to the preservation of those in whom the totality of the faculties has been most favourably moulded by functional changes.

Of course this sketch of the relations among the factors must be taken as in large measure a speculation. We are now too far removed from the beginnings of life to obtain data for anything more than tentative conclusions respecting its earliest stages; especially in the absence of any clue to the mode in which multiplication, first agamogenetic and then gamogenetic, was initiated. But it has seemed to me not amiss to present this general conception, by way of showing how the deductive interpretation harmonizes with the several inferences reached by induction.

In his article on Evolution in the *Encyclopædia Britannica*, Professor Huxley writes as follows :—

"How far 'natural selection' suffices for the production of species

> remains to be seen. Few can doubt that, if not the whole cause, it is a very important factor in that operation . . .
> On the evidence of palæontology, the evolution of many existing forms of animal life from their predecessors is no longer an hypothesis, but an historical fact; it is only the nature of the physiological factors to which that evolution is due which is still open to discussion."

With these passages I may fitly join a remark made in the admirable address Prof. Huxley delivered before unveiling the statue of Mr. Darwin in the Museum at South Kensington. Deprecating the supposition that an authoritative sanction was given by the ceremony to the current ideas concerning organic evolution, he said that "science commits suicide when it adopts a creed."

Along with larger motives, one motive which has joined in prompting the foregoing articles, has been the desire to point out that already among biologists, the beliefs concerning the origin of species have assumed too much the character of a creed; and that while becoming settled they have been narrowed. So far from further broadening that broader view which Mr. Darwin reached as he grew older, his followers appear to have retrograded towards a more restricted view than he ever expressed. Thus there seems occasion for recognizing the warning uttered by Prof. Huxley, as not uncalled for.

Whatever may be thought of the arguments and conclusions set forth in this article and the preceding one, they will perhaps serve to show that it is as yet far too soon to close the inquiry concerning the causes of organic evolution.

### NOTE.

[*The following passages formed part of a preface to the small volume in which the foregoing essay re-appeared. I append them here as they cannot now be conveniently prefixed.*]

Though the direct bearings of the arguments contained in this Essay are biological, the argument contained in its first half has indirect bearings upon Psychology, Ethics, and Sociology. My belief in the profound importance of these indirect bearings, was originally a chief prompter to set forth the argument; and it now prompts me to re-issue it in permanent form.

Though mental phenomena of many kinds, and especially of the simpler kinds, are explicable only as resulting from the natural selection of favourable variations; yet there are, I believe, still more numerous mental phenomena, including all those of any considerable complexity, which cannot be explained otherwise than as results of the inheritance of functionally-produced modifications. What theory of psychological evolution is espoused, thus depends on acceptance or rejection of the doctrine that not only in the individual, but in the successions of individuals, use and disuse of parts produce respectively increase and decrease of them.

Of course there are involved the conceptions we form of the genesis and nature of our higher emotions; and, by implication, the conceptions we form of our moral intuitions. If functionally-produced modifications are inheritable, then the mental associations habitually produced in individuals by experiences of the relations between actions and their consequences, pleasurable or painful, may, in the successions of individuals, generate innate tendencies to like or dislike such actions. But if not, the genesis of such tendencies is, as we shall see, not satisfactorily explicable.

That our sociological beliefs must also be profoundly affected by the conclusions we draw on this point, is obvious. If a nation is modified *en masse* by transmission of the effects produced on the natures of its members by those modes of daily activity which its institutions and circumstances involve; then we must infer that such institutions and circumstances mould its members far more rapidly and comprehensively than they can do if the sole cause of adaptation to them is the more frequent survival of individuals who happen to have varied in favourable ways.

I will add only that, considering the width and depth of the effects which acceptance of one or other of these hypotheses must have on our views of Life, Mind, Morals, and Politics, the question—Which of them is true? demands, beyond all other questions whatever, the attention of scientific men.

After the above articles were published, I received from Dr. Downes a copy of a paper "On the Influence of Light on Protoplasm," written by himself and Mr. T. P. Blunt, M.A., which was communicated to the Royal Society in 1878. It was a continuation of a preceding paper which, referring chiefly to *Bacteria*, contended that—

> "Light is inimical to, and under favourable conditions may wholly prevent, the development of these organisms."

This supplementary paper goes on to show that the injurious effect of light upon protoplasm results only in presence of oxygen. Taking first a comparatively simple type of molecule which enters into the composition of organic matter, the authors say, after detailing experiments:—

> "It was evident, therefore, that *oxygen* was the agent of destruction under the influence of sunlight."

And accounts of experiments upon minute organisms are followed by the sentence—

> "It seemed, therefore, that in absence of an atmosphere, light failed entirely to produce any effect on such organisms as were able to appear."

They sum up the results of their experiments in the paragraph—

> "We conclude, therefore, both from analogy and from direct experiment, that the observed action on these organisms is not dependent on light *per se*, but that the presence of free oxygen is necessary; light and oxygen
> accomplishing what neither can do alone: and the inference see
> that the effect produced is a gradual oxidation of the cor

of these organisms, and that, in this respect, protoplasm, although living, is not exempt from laws which appear to govern the relations of light and oxygen to forms of matter less highly endowed. A force which is indirectly absolutely essential to life as we know it, and matter in the absence of which life has not yet been proved to exist, here unite for its destruction."

What is the obvious implication? If oxygen in presence of light destroys one of these minutest portions of protoplasm, what will be its effect on a larger portion of protoplasm? It will work an effect on the surface instead of on the whole mass. Not like the minutest mass made inert all through, the larger mass will be made inert only on its outside; and, indeed, the like will happen with the minutest mass if the light or the oxygen is very small in quantity. Hence there will result an envelope of changed matter, inclosing and protecting the unchanged protoplasm—there will result a rudimentary cell-wall.

## A COUNTER-CRITICISM.

[*First published in* The Nineteenth Century, *for February*, 1888.]

WHILE I do not concur in sundry of the statements and conclusions contained in the article entitled "A Great Confession," contributed by the Duke of Argyll to the last number of this Review, yet I am obliged to him for having raised afresh the question discussed in it. Though the injunction "Rest and be thankful," is one for which in many spheres much may be said—especially in the political, where undue restlessness is proving very mischievous; yet rest and be thankful is an injunction out of place in science. Unhappily, while politicians have not duly regarded it, it appears to have been taken to heart too much by naturalists; in so far, at least, as concerns the question of the origin of species.

The new biological orthodoxy behaves just as the old biological orthodoxy did. In the days before Darwin, those who occupied themselves with the phenomena of life, passed by with unobservant eyes the multitudinous facts which point to an evolutionary origin for plants and animals; and they turned deaf ears to those who insisted on the significance of these facts. Now that they have come to believe in this evolutionary origin, and have at the
accepted the hypothesis that natural selec
sole cause of the evolution, they ar

of the multitudinous facts which cannot rationally be ascribed to that cause; and turn deaf ears to those who would draw their attention to them. The attitude is the same; it is only the creed which has changed.

But, as above implied, though the protest of the Duke of Argyll against this attitude is quite justifiable, it seems to me that many of his statements cannot be sustained. Some of these concern me personally, and others are of impersonal concern. I propose to deal with them in the order in which they occur.

On page 144 the Duke of Argyll quotes me as omitting "for the present any consideration of a factor which may be distinguished as primordial;" and he represents me as implying by this "that Darwin's ultimate conception of some primordial 'breathing of the breath of life' is a conception which can be omitted only 'for the present.'" Even had there been no other obvious interpretation, it would have been a somewhat rash assumption that this was my meaning when referring to an omitted factor; and it is surprising that this assumption should have been made after reading the second of the two articles criticised, in which this factor omitted from the first is dealt with: this omitted third factor being the direct physico-chemical action of the medium on the organism. Such a thought as that which the Duke of Argyll ascribes to me, is so incongruous with the beliefs I have in many places expressed that the ascription of it never occurred to me as possible.

Lower down on the same page are some other sentences having personal implications, which I must dispose of before going into the general question. The Duke says "it is more than doubtful whether any value attaches to the new factor with which he [I] desires to supplement it [natural selection]"; and he thinks it "unaccountable" that I "should make so great a fuss about so small a matter as the effect of use and disuse of particular organs as a separate and a newly-recognised factor in the development of varieties." I do not suppose that the Duke of Argyll intended to cast upon me the disagreeable imputation, that I claim as new that which all who are even slightly acquainted with the facts know to be anything rather than new. But his words certainly do this. How he should have thus written in spite of the extensive knowledge of the matter which he evidently has, and how he should have thus written in presence of the evidence contained in the articles he criticizes, I cannot understand. Naturalists, and multitudes besides naturalists, know that the hypothesis which I am represented as putting forward as new, is much older than the hypothesis of natural selection—goes back at least as far as Dr. Erasmus Darwin. My purpose was to bring into the foreground again a factor which has, I think, been of late years improperly ignored; to show that Mr. Darwin recognized this factor in an increasing degree as he grew older (by showing which I should have thought I sufficiently excluded the supposition that I brought it forward as new); to give further evidence that this factor is in operation; to show there are numerous phenomena which cannot be interpreted without it; and to argue that if proved operative in any case, it may be inferred that it is operative on all structures having active functions.

Strangely enough, this passage, in which I am represented as implying novelty in a doctrine which I have merely sought to emphasize and extend, is immediately succeeded by a passage in which the Duke of Argyll himself represents the doctrine as being familiar and well established :—

> "That organs thus enfeebled [i.e. by persistent disuse] are transmitted by inheritance to offspring in a like condition of functional and structural decline, is a correlated physiological doctrine not generally disputed. The converse case—of increased strength and development arising out of the habitual and healthy use of special organs, and of the transmission of these to offspring—is a case illustrated by many examples in the breeding of domestic animals. I do not know to what else w [illegible] lender legs and bodies of greyhounds so manifes [illegible] he delicate powers

**of smell in pointers and setters, or a dozen cases of modified structure effected by artificial selection."**

In none of the assertions contained in this passage can I agree. Had the inheritance of "functional and structural decline" been "not generally disputed," half my argument would have been needless; and had the inheritance of "increased strength and development" caused by use been recognized, as "illustrated by many examples," the other half of my argument would have been needless. But both are disputed; and, if not positively denied, are held to be unproved. Greyhounds and pointers do not yield valid evidence, because their peculiarities are more due to artificial selection than to any other cause. It may, indeed, be doubted whether greyhounds use their legs more than other dogs. Dogs of all kinds are daily in the habit of running about and chasing one another at the top of their speed—other dogs more frequently than greyhounds, which are not much given to play. The occasions on which greyhounds exercise their legs in chasing hares, occupy but inconsiderable spaces in their lives, and can play but small parts in developing their legs. And then, how about their long heads and sharp noses? Are these developed by running? The structure of the greyhound is explicable as a result mainly of selection of variations occasionally arising from unknown causes; but it is inexplicable otherwise. Still more obviously invalid is the evidence said to be furnished by pointers and setters. How can these be said to exercise their organs of smell more than other dogs? Do not all dogs occupy themselves in sniffing about here and there all day long: tracing animals of their own kind and of other kinds? Instead of admitting that the olfactory sense is more exercised in pointers and setters than in other dogs, it might, contrariwise, be contended that it is exercised less; seeing that during the greater parts of their lives they are shut up in kennels where the varieties of odours, on which to practise their noses, is but small. Clearly if breeders of sporting dogs have from early days habitually bred from those puppies of each litter which had the keenest noses (and it is undeniable that the puppies of each litter are made different from one another, as are the children in each human family, by unknown combinations of causes), then the existence of such remarkable powers in pointers and setters may be accounted for; while it is otherwise unaccountable. These instances, and many others such, I should have gladly used in support of my argument, had they been available; but unfortunately they are not.

On the next page of the Duke of Argyll's article (page 145), occurs a passage which I must quote at length before I can deal effectually with its various statements. It runs as follows:—

> "But if natural selection is a mere phrase, vague enough and wide enough to cover any number of the physical causes concerned in ordinary generation, then the whole of Mr. Spencer's laborious argument in favour of his 'other factor' becomes an argument worse than superfluous. It is wholly fallacious in assuming that this 'factor' and 'natural selection' are at all exclusive of, or even separate from, each other. The factor thus assumed to be new is simply one of the subordinate cases of heredity. But heredity is the central idea of natural selection. Therefore natural selection includes and covers all the causes which can possibly operate through inheritance. There is thus no difficulty whatever in referring it to the same one factor whose solitary dominion Mr. Spencer has plucked up courage to dispute. He will never succeed in shaking its dictatorship by such a small rebellion. His little contention is like some bit of Bumbledom setting up for Home Rule—some parochial vestry claiming independence of a universal empire. It pretends to set up for itself in some fragment of an idea. But here is not even a fragment to boast of or to stand up for. His new factor in organic evolution has neither independence nor novelty. Mr. Spencer is able to quote himself as having mentioned it in his *Principles of Biology* published some twenty years ago; and by a careful ransacking of Darwin he shows that the idea was familiar to and admitted by him at least in his last edition of the *Origin of Species*. . . . Darwin was a man so much wiser than all his followers," &c.

Had there not been the Duke of Argyll's signature to the article, I could scarcely have believed that this passage was written by him. Remembering that on reading his article in the preceding number of this Review, I was struck by the extent of knowledge, clearness of discrimination, and power of exposition, displayed in it, I can scarcel understand how there has come from the same pen passage in which none of these traits are exhibited. Eve one wholly unacquainted with the subject may see in th last two sentences of the above extract, how strangel its propositions are strung together. While in the first o them I am represented as bringing forward a "new factor,' I am in the second represented as saying that I mentione it twenty years ago! In the same breath I am described a claiming it as new and asserting it as old! So, again, th uninstructed reader, on comparing the first words of th extract with the last, will be surprised on seeing in scientific article statements so manifestly wanting in precision. If "natural selection is a mere phrase," how ca Mr. Darwin, who thought it explained the origin of specie be regarded as wise? Surely it must be more than a mer phrase if it is the key to so many otherwise inexplicabl facts. These examples of incongruous thoughts I give t prepare the way; and will now go on to examine the chie propositions which the quoted passage contains.

The Duke of Argyll says that "heredity is the centra idea of natural selection." Now it would, I think, be concluded that those who possess the central idea of a thin have some consciousness of the thing. Yet men have possessed the idea of heredity for any number of generation and have been quite unconscious of natural selectio Clearly the statement is misleading. It might just as trul be said that the occurrence of structural variations i organisms is the central idea of natural selection. And i might just as truly be said that the action of externa agencies in killing some individuals and fostering others i the central idea of natural selection. No such assertion are correct. The process has three factors—heredity variation, and external action—any one of which bein absent, the process ceases. The conception contains thre corresponding ideas, and if any one be struck out, the conception cannot be framed. No one of them is the central idea, but they are co-essential ideas.

From the erroneous belief that "heredity is the central idea of natural selection" the Duke of Argyll draws the conclusion, consequently erroneous, that "natural selection includes and covers all the causes which can possibly operate through inheritance." Had he considered the cases which, in the *Principles of Biology*, I have cited to illustrate the inheritance of functionally-produced modifications, he would have seen that his inference is far from correct. I have instanced the decrease of the jaw among civilized men as a change of structure which cannot have been produced by the inheritance of spontaneous, or fortuitous, variations. That changes of structure arising from such variations may be maintained and increased in successive generations, it is needful that the individuals in whom they occur shall derive from them advantages in the struggle for existence—advantages, too, sufficiently great to aid their survival and multiplication in considerable degrees. But a decrease of jaw reducing its weight by even an ounce (which would be a large variation), cannot, by either smaller weight carried or smaller nutrition required, have appreciably advantaged any person in the battle of life. Even supposing such diminution of jaw to be beneficial (and in the resulting decay of teeth it entails great evils), the benefit can hardly have been such as to increase the relative multiplication of families in which it occurred generation after generation. Unless it has done this, however, decreased size of the jaw cannot have been produced by the natural selection of favourable variations. How can it then have been produced? Only by decreased function—by the habitual use of soft food, joined, probably, with disuse of the teeth as tools. And now mark that this cause operates on all members of a society which falls into civilized habits. Generation after generation this decreased function changes its component families simultaneously. Natural selection does not cover the case at all—has nothing to do with it. And the like happens in multitudinous other cases. Every species spreading into a new habitat, coming in contact with new food, exposed to a different temperature, to a drier or moister air, to a more irregular surface, to a new soil, &c., &c., has its members one and all subject to various changed actions, which influence its muscular, vascular, respiratory, digestive, and other systems of organs. If there is inheritance of functionally-produced modifications, then all its members will transmit the structural alterations wrought in them, and the species will change as a whole without the supplanting of some stocks by others. Doubtless in respect of certain changes natural selection will co-operate. If the species, being a predacious one, is brought, by migration, into the presence of prey of greater speed than before; then, while all its members will have their limbs strengthened by extra action, those in whom this muscular adaptation is greatest will have their multiplication furthered; and inheritance of the functionally-increased structures will be aided, in successive generations, by survival of the fittest. But it cannot be so with the multitudinous minor changes entailed by the modified life. The majority of these must be of such relative unimportance that one of them cannot give to the individual in which it becomes most marked, advantages which predominate over kindred advantages gained by other individuals from other changes more favourably wrought in them. In respect to these, the inherited effects of use and disuse must accumulate independently of natural selection.

To make clear the relations of these two factors to one another and to heredity, let us take a case in which the operations of all three may be severally identified and distinguished.

Here is one of those persons, occasionally met with, who has an additional finger on each hand, and who, we will suppose, is a blacksmith. He is neither aided nor much hindered by these additional fingers; but, by constant use, he has greatly developed the muscles of his right arm. To avoid a perturbing factor, we will assume that his wife, too, exercises her arms in an unusual degree: keeps a mangle, and has all the custom of the neighbourhood. Such being the circumstances, let us ask what are the established facts, and what are the beliefs and disbeliefs of biologists.

The first fact is that this six-fingered blacksmith will be likely to transmit his peculiarity to some of his children; and some of these, again, to theirs. It is proved that, even in the absence of a like peculiarity in the other parent, this strange variation of structure (which we must ascribe to some fortuitous combination of causes) is often inherited for more than one generation. Now the causes which produce this persistent six-fingeredness are unquestionably causes which "operate through inheritance." The Duke of Argyll says that "natural selection includes and covers all the causes which can possibly operate through inheritance." How does it cover the causes which operate here? Natural selection never comes into play at all. There is no fostering of this peculiarity, since it does not help in the struggle for existence; and there is no reason to suppose it is such a hindrance in the struggle that those who have it disappear in consequence. It simply gets cancelled in the course of generations by the adverse influences of other stocks.

While biologists admit, or rather assert, that the peculiarity in the blacksmith's arm which was born with him is transmissible, they deny, or rather do not admit, that the other peculiarities of his arm, induced by daily labour—its large muscles and strengthened bones—are transmissible. They say that there is no proof. The Duke of Argyll thinks that the inheritance of organs enfeebled by disuse is "not generally disputed;" and he thinks there is clear proof that the converse change—increase of size consequent on use—is also inherited. But biologists dispute both of these alleged kinds of inheritance. If proof is wanted, it will be found in the proceedings at the last meeting of the British Association, in a paper entitled "Are Acquired Characters Hereditary?" by Professor Ray Lankester, and in the discussion raised by that paper. Had this form of inheritance been, as the Duke of Argyll says, "not generally disputed," I should not have written the first of the two articles he criticizes.

But supposing it proved, as it may hereafter be, that such a functionally-produced change of structure as the blacksmith's arm shows us, is transmissible, the persistent inheritance is again of a kind with which natural selection has nothing to do. If the greatly strengthened arm enabled the blacksmith and his descendants, having like strengthened arms, to carry on the battle of life in a much more successful way than it was carried on by other men, survival of the fittest would ensure the maintenance and increase of this trait in successive generations. But the skill of the carpenter enables him to earn quite as much as his stronger neighbour. By the various arts he has been taught, the plumber gets as large a weekly wage. The small shopkeeper by his foresight in buying and prudence in selling, the village-schoolmaster by his knowledge, the farm-bailiff by his diligence and care, succeed in the struggle for existence equally well. The advantage of a strong arm does not predominate over the advantages which other men gain by their innate or acquired powers of other kinds; and therefore natural selection cannot operate so as to increase the trait. Before it can be increased, it is neutralized by the unions of those who have it with those who have other traits. To whatever extent, therefore, inheritance of this functionally-produced modification operates, it operates independently of natural selection.

One other point has to be noted—the relative importance of this factor. If additional developments of muscles and bones may be transmitted—if, as Mr. Darwin held, there are various other structural modifications caused by use and disuse which imply inheritance of this kind—if acquired characters are hereditary, as the Duke of Argyll believes; then the area over which this factor of organic evolution operates is enormous. Not every muscle only, but every nerve and nerve-centre, every blood-vessel, every viscus, and nearly every bone, may be increased or decreased by its influence. Excepting parts which have passive functions, such as dermal appendages and the bones which form the skull, the implication is that nearly every organ in the body may be modified in successive generations by the augmented or diminished activity required of it; and, save in the few cases where the change caused is one which conduces to survival in a pre-eminent degree, it will be thus modified independently of natural selection. Though this factor can operate but little in the vegetal world, and can play but a subordinate part in the lowest animal world; yet, seeing that all the active organs of all animals are subject to its influence, it has an immense sphere. The Duke of Argyll compares the claim made for this factor to "some bit of Bumbledom setting up for Home Rule—some parochial vestry claiming independence of a universal empire." But, far from this, the claim made for it is to an empire, less indeed than that of natural selection, and over a small part of which natural selection exercises concurrent power; but of which the independent part has an area that is immense.

It seems to me, then, that the Duke of Argyll is mistaken in four of the propositions contained in the passages I have quoted. The inheritance of acquired characters *is* disputed by biologists, though he thinks it is not. It is not true that "heredity is the central idea of natural selection." The statement that natural selection includes and covers all the causes which can possibly operate through inheritance, is quite erroneous. And if the inheritance of acquired characters is a factor at all, the dominion it rules over is not insignificant but vast.

Here I must break off, after dealing with a page and a half of the Duke of Argyll's article. A state of health which has prevented me from publishing anything since "The Factors of Organic Evolution," now nearly two years ago, prevents me from carrying the matter further. Could I have pursued the argument it would, I believe, have been practicable to show that various other positions taken up by the Duke of Argyll do not admit of effectual defence. But whether or not this is probable, the reader must be left to judge for himself. On one further point only will I say a word; and this chiefly because, if I pass it by, a mistaken impression of a serious kind may be diffused. The Duke of Argyll represents me as "giving up" the "famous phrase" "survival of the fittest," and wishing "to abandon it." He does this because I have pointed out that its words have connotations against which we must be on our guard, if we would avoid certain distortions of thought. With equal propriety he might say that an astronomer abandons the statement that the planets move in elliptic orbits, because he warns his readers that in the heavens there exist no such things as orbits, but that the planets sweep on through a pathless void, in directions perpetually changed by gravitation.

I regret that I should have had thus to dissent so entirely from various of the statements made, and conclusions drawn, by the Duke of Argyll, because, as I have already implied, I think he has done good service by raising afresh the question he has dealt with. Though the advantages which he hopes may result from the discussion are widely unlike the advantages which I hope may result from it, yet we agree in the belief that advantages may be looked for.

END OF VOL. I.

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